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PART FIRST.

ORIGINAL COMMUNICATIONS.

ART. I.—*An account of a new species of Eruptive Fever, which prevailed in Willoughby, Lake county, Ohio, during the Spring, Fall and Winter of* 1853.—By F. C. GIBBS, *of Willoughby.*

About the middle of April, two persons living three miles apart, and having had no intercourse with each other, and living on roads having no connection by ordinary travel, were attacked with the disease, an account of which we are about to give. One of the cases we saw from the beginning. In due time, it proved to be an eruptive fever, bearing considerable resemblance to severe chicken pox, but still more to small pox. For the first eight days, we were engaged in making observations and comparisons, having in view the true diagnosis of the disease. In due time, a critical examination, external and internal, of the vesicles, made us certain that whatever might be the nature of the poison, it was not variolous. The other case we did not see; but a second case occurring in the family two weeks subsequently, to which we were called, enabled us to become certain that the two diseases were the result of the same specific poison. Synchronous with this third case, two others occurred three miles distant, and a mile from each other. More or less of the members of the families in which the disease had made ingress, became affected, and new cases sprang up during the season,

from time to time, in persons who had been in no way exposed to the sick; so that by the middle of November, at which time the disease disappeared, there had been in my practice, twelve cases contracted without, and some forty-seven in consequence of, exposure to the disease. It was noticed that most, if not all those who contracted the disease without exposure, were in *ill health*, or otherwise laboring under embarrassments, favorable to the contracting of disease. A very few cases occurred besides those under my charge. With these preliminary remarks, we proceed to a description of the disease, leaving some details of the history to be supplied in their proper places:

This malady, in its usual form, is characterized by a regular succession of symptoms, and these occur in the order and form to be mentioned; and it has a tendency to a specific termination. The symptoms, for convenience of description, and a correct appreciation of the disease, we shall divide into the following series:

1. Period of Invasion.
2. Period of Eruption.
3. Period of Despumation.
4. Period of Passivity.
5. Period of Desquamation.

Period of Invasion.—This stage is characterized by a feeling of prostration, slight chills, more or less, (but generally moderate,) and pain in the back and limbs. Head ache of a peculiar character, to-wit: Dull pain, with a feeling of insupportable weight, with dizziness and swimming. The skin in several cases may be hot, and more or less dry—tongue white—pulse accelerated. Nausea, and occasionally, though rarely, vomiting. If the disease is to run a benignant course, the skin may be simply dry and husky, but not at all remarkable for heat; the pulse but little accelerated, slight distress at the stomach, and the tongue not sensibly affected. This state of the system continuing till the eruption is fully established, when some amelioration of the system ensues.

Period of Eruption.—The eruptive stage is ushered in by the appearance of red points on the forehead. These extend over the face, breast, body and limbs, in the order enumerated, and usually reach the feet within twenty-four hours; but they increase in number regularly, in such wise that forty-eight hours are occupied in the completion of the process. If, however, the disease is to be severe, and the eruption dense, the eruption, after its appearance on the

forehead, remains stationary for twenty-four hours, and then proceeds over the system as before, reaching the feet in forty-eight instead of twenty-four hours. The eruption, in the more severe cases, appears on the second day—on the third in a few, but in all medium cases, on the fourth day. In the slightest form of the disease, the eruption is delayed to the fifth, sixth and even the eighth day. During this stage, the vesicles become fully developed in form and size, requiring full seven days for each vesicle, or from nine to ten days from the appearance of the first, to the full developement of the last vesicle. The appearance of the eruption differs according to the part of the system it occupies. Thus, on the face, the eruption is papular, and upon each papulæ, at the end of the second day, a vesicle is formed. But in those severe cases, where the eruption remains stationary for twenty-four hours, this change is not consummated till the third day. On the body and extremities, as far down as the ankles and wrists, the eruption commences with dark red points. These gradually enlarge, having no elevation apparent to the eye, but having an indurated feel to the finger. On each of them, on the second day of their appearance, a vesicle is formed. On the hands and feet, invariably on the former, the eruption consists in oval vesicles, distended with colorless lymph. All the vesicles continue gradually to enlarge for seven days. The form which the vesicles assume during this stage, depends exclusively on the quantity of fluids contained in them. If this be small, they commence becoming *indented*—if more, *flattened*, or slightly oval—if abundant, *hemispherical*. But they never acquire an indented figure during their progress. Whatever be the *form* of the vesicles, the fluid in them has a uniform appearance. It is, during the whole stage, perfectly limpid and colorless. The diameter of the vesicles is from an eighth to a sixth of an inch. After the eruption is fully established, the fever usually subsides, and the system remains tolerably quiet, till the vesicles acquire such a size, and inflammatory action, as to induce a sympathetic fever. The severity of this, depends on the denseness of the eruption. If this be slight, little or no fever will be induced; if very abundant, the fever will run high. The fever usually commences about the fifth or sixth day. The eruption spreads upon the mucous membranes, and is indicated by the appearance on these surfaces of white points slightly elevated, but containing no fluid. They are first perceived about the fifth day of the eruption. In moderate cases, they produced no special disturbance,

but in some cases, about the sixth day of the eruption, the throat becomes sore and painful, and more or less cough comes on, with expectoration of tenacious mucous, and thus ends the period of eruption.

Period or Despumation.—This stage comprehends the period during which the fluid in the vesicles comes to maturity. The term despumation, is regarded as most expressive of this occurrence, for, whether it be literally true, as Sydenham says, an "imposthume is an instrument of nature, whereby she expels whatever is injurious," or that a change similar to this is "a fermenting process, whereby the blood is clarified," as saith Dr. Richard Mead; or Liebig, who, speaking of certain poisons, says: "When introduced into the blood of a healthy individual, it will be again generated in the blood, just as yeast is re-produced from wort." Whatever be the theory, it is certain that after the vesicles are filled to the utmost capacity, a change in the character of the fluid is necessary to a restoration of harmony to the system. This change consists in the conversion of *limpid transparent lymph*, to *semi-viscid turbid lymph*. As soon as this change is effected, which commences on the eighth, and is completed on the tenth day, harmony is restored to the system. The fever subsides—pulse returns to its usual frequency and qualities, the tongue is freed from its coat; the urine becomes normal; the appetite returns; in short there is an apparent, if not a literal clarification.

While these changes are going on within the vesicles, the cuticle covering them, and constituting their superior walls, changes to a yellow color. Their form is hemispherical. The face is more or less swollen, and at times, the eyes inflamed; so of the hands and fingers, especially if the cuticle be thick. The cough and expectoration continue, if they be present, through this stage. At the end of the tenth day, the change being effected, this stage is closed.

Complications.—It occasionally happens in this disease, that on the face, the vesicles stand so closely together, that when developed, they crowd each other so closely, that the walls between them are ruptured, and hence, they form one consolidated vesicle. When the same thing happens in chicken pox and small pox, they are said to be confluent, and treated as a separate variety. But as this condition is from an accidental circumstance, we regard it as a complication of the ordinary disease. In this case, the whole cuticle on the face being destroyed, much inflammation is induced. It is in these

cases more particularly that the mucous membrane of the tongue, and parts contiguous, is involved to a greater extent and considerable prostration is at times induced. If the patient be of good constitution, however, the eruption on other parts of the system, goes through its regular changes, in the usual periods of time, and terminates as in the uncomplicated cases. But if he be feeble, or in infancy, the appropriate changes are retarded, the vesicles remaining nearly stationary for several days, during which, prostrative and typhoid symptoms supervene; when, finally, if the disease is to terminate favorably, the change is effected and the formidable symptoms slowly subside. In the few cases that have terminated fatally, the vesicles have remained unchanged, continuing limpid and colorless. This consolidated vesicle contains but a small quantity of lymph, and though it is more tenacious than in the vesicles on the body, still it is so limpid that most of it, within a short time, escapes through the rents in the cuticle, leaving the latter with whatever lymph may still adhere to it, on the twelfth day, lying flat on the surface, *in patches*. These become partially dry, and are removed as early as the fifteenth day, leaving the surface more or less abraded, to be restored during the period of desquamation.

Passive Stage.—Returning to the uncomplicated cases, we repeat that at the end of the period of despumation, the active symptoms of the disease will have subsided. But the vesicles are still distended with lymph, and health cannot be restored while these remain; and if left to themselves, they remain many days stationary—the external appearance, and the internal fluidity, seeming to have assumed a perfect statu quo. Hence, we denominate this the stage of passivity. The only appreciable change in the vesicles, for several days, is, that they become slightly more limpid. The length of time which the fluids remain, will depend on the thickness of the cuticle, and the part of the body on which they are. Thus, on the back they are generally ruptured as early as the twelfth day, in consequence of the recumbent position, and a disposition on the part of the patient to rub, with a view to relieve the itching. On the abdomen, arms and legs, they will remain to the fifteenth or twentieth day; and on the hands and feet, much longer, still retaining their fluidity. The vesicles finally burst and the fluid escapes, leaving the derma exposed, but opening a way for recovery during the period following. This is the rule, but occasionally the lymph becomes congealed into a thin scale, the unbroken cuticle still remaining.

This stage may be cut short many days by rupturing the vesicles and allowing the fluids to escape, and thus bringing on the

Desquamation Period.—A peculiarity of this disease is, that the fluids do not dry up and form a scab, but escape, leaving a base excoriated, which, like an ordinary abraded surface, after two or three days, is covered by a scale or thin scab, which, after a time, falls off, either leaving the surface restored, or to be succeeded by another, or a series of them, till a cure is affected. In the ordinary cases, the derma not having been injured, no permanent marks will be left, but if, as is frequently the case, it be injured, it may be otherwise. In the other cases, after the cuticle is removed, a succession of scales succeed it, leaving the face more or less rough and uneven, and when it has been much ulcerated, disfigured by cicatrices. In two or three cases, more or less injury to one eye has been produced from the disease, and small abscesses have occasionally been noticed; but generally, this affection seems to have no tendency, permanently to impair the constitution, or to be succeeded by disease in any form.

Irregular forms of the Disease.—During the progress of the disease, we have occasionally found imperfectly developed cases, wanting in more or less of the ordinary symptoms of the disease, but which are capable of imparting to others the genuine disease, and hence require a cursory notice:

1. We find cases having all the symptoms peculiar to the invasive stage, when, on the third day, instead of the ordinary eruption, and efflorescence breaks out, first over the forehead, face and breast, and within twenty-four hours, spreads over the system. The eruption in these cases is not unlike that modification of rubeola, known as rubeola sine catarrho. It continues three or four days, the febrile symptoms subsiding with the receeding of the efflorescence.

2. Again we find the eruption stage passing along mildly, but in the usual manner, no eruption appearing till five, six, or even till the eighth day, when finally, a few pimples appear here and there over the body, and synchronously therewith, the febrile symptoms subside, and the system returns to its natural condition.

3. Still others pass through the first stage as usual, and on the fourth day, the eruption breaks out, papular, and on a more narrow base, than the ordinary disease. On the second day of the eruption, a vesicle arises on the apex of each, very small and globular. They slowly fill, retaining their original form, and coming to maturity on

the sixth or seventh day. The matter in them is more tenacious and yellow, than in the ordinary disease, and they more frequently desicate. We noticed but few of these cases. They seem to result from idiosyncrasy in those persons affected by the disease.

Pathology.—No post mortem examinations have been made. We have given the external appearance of the vesicles. On the internal we now speak. The location of the vesicles, is between the cuticle and the rete mucosum. The lympth, through all stages of the disease, is in a single cavity. A puncture at any point will permit all of the fluid to escape. On making a transverse section of a vesicle, at any stage of the disease, and wiping away the lymph, a single cup like cavity will remain. About the twelfth or thirteenth day of the disease, the rete mucosum separates from the derma, and after floating loose in the cavity of the vesicles for a day or two, rises and becomes attached to the cuticle. The bottom of the cavity seen after this separation, presents a uniform level, showing no appearance of ulceration, or a central point. A careful and vigilant observation, shows that no fluid is deposited under the membrane previous to its separation.

Period of Incubation.—This is two weeks. We have never found it less, and though it may at times be more, there is no certain evidence of it. It is known that the disease may be contracted during the period of invasion, and the probabilities are, that it is generally contracted during that stage; but it may, perhaps, also be imparted at other stages of this malady.

Diagnosis.—This malady can be mistaken for no disease except chicken pox, or small pox; and while the milder cases most resemble the former, the more severe have been mistaken for the latter, both by persons in, and out of the medical profession. And as the writer has strong reasons for believing that the disease has prevailed in more places than one in our country, and that to a greater or less extent, its true nature and relations have not been appreciated by all, the indulgence of the reader is solicited, while we speak somewhat in detail of the diagnostic symptoms, which separate the disease from small pox. They are unlike in respect to the following particulars: In the period of incubation—in the time at which the eruptions appear—in the symptoms during the period of invasion—in the form of the eruption during the early and middle periods of the eruptive stage—in the anatomical arrangement during the same period—in the time required for the two diseases respectively, to come

to maturity—in the external appearance of the two immediately after the maturing period—in the quality of the contents of the two, *at* the maturing period—in respect to the material out of which the scab is formed—in the time required for the removal of the crust upon the face in the complicated or confluent cases—and in the location of the two diseases in respect to the layers of the skin. These distinctions will be more readily brought to view in the following arrangement:

Recent Eruptive Fever.	Small Pox.
1. Period of incubation, *two weeks.* Not certain; but it may be longer in some cases, but certainly never less.	1. Morely's observation, 8 *days;* Gregory puts it at from 10 *to* 14 *days;* Watson 12 *days,* Wilson, from 6 to 20 days—being *variable but less* in time.
2. Duration of precursory symptoms, from *two to four* days, and in some cases, *five, six,* or more days.	2. Precursory symptoms vary little from *three days*—"severe cases *early third* day—moderate cases *late* on the *third day.*" (Cullen.)
3. Seizure *gradual,* vomiting rarely; sense of *weight and swimming* always; moderate *pain in back* and limbs—no cramps in *early* stage, or convulsions in *any stage.*	3. "Seizure *sudden,*" (Bartlet.) "Nausea vomiting and intense pain in the back." (Chornel.) "Cough and convulsions frequent during this stage." (Wilson.)
4. The vesicles are in a single cavity. A single punctnre will remove all the fluids, if pressed. A transverse section being made, the remaining portion will be a *single cup.*	4. The pock "exhibits in its early stages, a *cellular* structure." (Gregory.) "If during this stage, a transverse section be made, the vesicle will show a cellular structure, and its appearance will be like a severed orange." (Bousquet.)
5. The vesicles never pass from an oval form into a depressed, indented or umbilical figure.	5. The elevated papulæ on the third, fourth and fifth days, *pass into a depressed, indented or umbilical form.* (Wilson.)
6. The vesicles come to maturity on the tenth day of the eruption.	6. Maturity is accomplished on the eighth day. (Watson.) "On the eighth day the suppuration process is completed." (Bartlet.)

7. After coming to maturity, the vesicles remain in a passive state for several days, the *yellow* external, and the internal fluidity remaining unchanged.

7. About the eighth day, a dark spot makes its appearance on the top of each turgid postule, (Watson,) and *at that time commences* the drying up of the pus and pustular discharges, and the conversion of these *fluids into scabs.* (Wilson.)

8. The change at the period of maturity consists in the conversion of the fluid in the vesicles, into semi-viscid whey-colored lymph.

8. "There is a conversion of the contained lymph into pus." (Wilson.)

9. When the vesicles are ruptured, the *lymph being limpid, escapes,* and *some days afterward,* thin scales or scabs are formed on the abraded surfaces.

9. Desiccation is indicated by the drying up of the pus, and purulent discharges, produced during the period of eruption, and by the *conversion of these fluids into scabs.* (Wilson.)

10. The vesicles are situated between the cuticle and the rete muscosum.

10. Velpeau and Herming believe its seat to be in the follicles of the derma, and all believe it at least as deep as the rete mucosum.

11. The vesicles through every stage contain but one cavity, and are never divided by a transverse membrane.

11. When a well formed, mature pustule is formed by dissection, it will be found to be divided in its interior by a transverse membrane, forming *two chambers,* both containing pus. (Wilson.)

12. In the complicated cases, the face is always cleared of its cuticle within five days of the period of maturity. Does not form a consolidated scab, but lies in patches, and is detached in separate portions.

12. "In the severe cases of this kind, the crust forms a complete mask to the features, and remains attached for ten or twelve days." (Wilson.)

The above are only some of the more important differences which exist between the disease we are speaking of, and Small Pox. We might pursue the contrast still further, but we believe the foregoing to be sufficient to satisfy the mind of every individual who has a just appreciation of the laws of diagnosis.

Is it a Modification of Small Pox?—To answer this query, it may be well to inquire by what causes small pox may be modified. It may be modified 1st by innoculation of the variolous poison. 2. By the previous subjugation of the system to cow pox. 3d. By person idiocracy. 4. By a previous attack of small pox.

We can now answer that the malady has not been modified by innoculation, or small pox, because but six of these have been exposed to our epidemic, and though three of them had it, one in its worst form, and two mildly, yet the history of these cases in no way varies from the cases of corresponding severity in those who were entirely unprotected by the variolous poison. It is clear that though personal idiocracy may affect individual cases, it cannot change a whole epidemic, and no two cases modified by this cause would be identical.

Lastly; is the disease that modification of small pox known as Varioloid? Two facts show that the thing is impossible. 1st. There has not been the slightest anatomical difference between the vaccinated and the unvaccinated cases, and so of the mild cases. 2d. Only a certain proportion of the cases had been subjected to the cow pox virus. The others, of course, could not have been varioloid.

If, then, the disease be a modification of small pox, it is a modification hitherto unknown. But, that it is not even such a modification, we are inclined to believe, for the following reasons: A disease which is but a modification of another, has no persistent constitution of its own. For example—Varioloid is a modification of variola, and it has three or four distinct kinds of eruption, which in an epidemic occur with perhaps equal frequency, and these varieties intermingle in unending succession. Again, modified forms of a disease usually occur at the same places where, and cotemporaneous with the disease, of which they are a modification—thus the Buffalo and Philadelphia Journals show that in sundry places, small pox has recently prevailed, and that it was accompanied by many modified cases—but there *a proportion of the cases were true small pox*, and hence the unique cases were easily understood. On the contary, in our epidemic, this disease has a constitution of its own—a very large proportion of the cases going though their regular stages in regular succession, in uniform periods of time, and terminating in a specific manner. The modified cases not being in greater proportion than what is usual in kindred affections, hence we regard ourselves justified in placing this malady in the catalogue of ills as a new disease.

Prognosis.—A few words will suffice on this point. A favorable termination may be expected if the patient have passed the period of infancy and is of good constitution. Five cases out of sixty have proved fatal, but two were infants, one of three months. One had disease of the heart, one a fractured femur, and one may be traced to great indiscretion on the part of the patient. Still the complicated cases are fearful, and to some extent compromising both life, limb and features, and deserve serious consideration.

Can it be a New Disease? (!)—To most persons, the announcement of a new disease, like a new discovery in science, is received with little favor. We are accustomed to think that all the physician has to do in diagnosis, is to refer to the books, where we suppose all diseases to which the system has been, or can be subjected are arranged in Nosological order! And Physicians, too, often think in the same circle. For example, an eruptive fever, like the one we are treating, occurs. It resembles chicken pox, and especially smallpox. "Well," soliloquizes the Doctor, "it is true the disease *resembles* chickenpox, but it is too severe for that disease, and it must be either chicken or small pox, ergo, it is small pox." The probability, or possibility, of a new disease is not entertained, and hence, diseases radically different are huddled together, pell mell, in "confusion worse confounded." For ourselves, we object to this method of jumping at conclusions as alike detrimental to the interests of a just pathology, or successful practice. For, why should it be thought anything incredible that a new disease should arise? Was such a thing never known? Did not Procohises, in the sixth century, speak of a new disease "for which no reason could be given but the will of God?" Was not Measles a new disease in the tenth century, and Scarlet fever in the fifteenth? Where was the Sudon Anglicanus till seen and described by Caius in 1551? —Or the Dengue before 1780, and the Epidemic Cholera before the eighteenth century? Does the reader remember the mortal colic which depopulated Spain in 1600, a disease never known before or since? But why should we particularize? New diseases are thick around us, and no physician has practised his profession successfully who is not aware of the fact. And it is just what every man ought to expect, who is alive to the changes that are going on around us. Impurity pervades earth and its appurtenances. Water, so essential to our existence, contains ever varying sources of impurity. Carbonic acid from the air and mineral poisons held in solution, and

evaporation from the surface of the land, carry with it subtle poisons, which are constantly entering our system. The effect of electricity, not only the learned, but thinking men everywhere, are now aware, is not confined to those rare periods when—

"Wide bursts in dazzling sheets the liquid flame,
And dread convulsion rends the etherial frame,"

but its evolution from water, and its constant presence in the atmosphere, is recognized as producing constant and varying impressions on the body and mind. Hence as we are having new impressions made upon us, it is to be expected that new effects will be the result.

When we speak of new diseases, however, we do not wish to be understood as asserting that they are absolutely new; they may have previously existed, but if so, those who saw them failed to describe them with accuracy; and hence they are *new* to us. Now, when such diseases first appear, they are very frequently mistaken for the diseases they most resemble—thus measles was for a time confounded with smallpox, scarlatina with rubeola, and cholera with cholera morbus, and our eruptive fever will, among superficial observers, be mistaken for variola. Indeed the resemblance between cholera and cholera morbus is much closer than between our disease and small pox. In both there is profuse vomiting, in both copious purging, in both cramp in the legs and abdominal muscles, in both great prostration and a tendency to collapse.

Whether our epidemic be produced by a new epidemic tendency, or the mingling together of the specific causes of two or more kindred diseases, it is as unnecessary to inquire as it is impossible to decide. Be this as it may, it would seem that similar diseases have been witnessed, though imperfectly described; and they too, have been mistaken for small pox. Thus Dr. Bartlett says:—"We sometimes witness a singular mixture of these diseases, when the greater size of the pustules, the increase of the local inflammation, and the general fever, have lead to the disease being mistaken for small pox, which formerly led to the mischievous error of putting the sufferer off his guard against that disease. Sydenham also mentions a disease which prevailed in his time, *called* small pox, which was not the regular disease. He thus speaks of it: "The small pox which prevailed during this epidemic, spared none, not even allowing those to escape who had *had the bastard* kind, which is in no way akin to that disease."

That our disease is from one or the other of these causes, there seems to be no room to doubt, by those who examine things by the light of nature, *as it is.*

We acknowledge that there are some *very old* opinions with which our views do not harmonize. Mr. A., a disciple of ancient memory, contends that all diseases are as old as the world, and are the result

> "Of man's first disobedience, and the fruit
> Of that forbidden tree, what mortal taste
> Brought death into the world, and all our woe."

while Mr. B. contends that the solution of this gordian knot is to be found in the less grave, but more artful, and equally scholastic philosophy, found hidden away among the mouldering archives which take hold of those ages which reach ancient Mythology. Here we find the mysterious box which Jupiter gave to Pandora, and she to her son Epimetheus, and through whose unpardonable inquisitiveness *all the raging distempers we now feel* have spread over the earth!

But Mr. C., a disciple of Homer, assures us that he has learned that—

> "On mules and dogs the infection first began,
> And last the vengeful arrows fell on man."

We shall not at present wait to inquire to which of the above theories they incline, who think the opinion of a new disease incredible, but proceed as advocates of an inductive philosophy, to speak of the

CAUSE OF THE DISEASE.

The history of the origin of the disease demonstrates its atmospheric cause. The first two cases occurring synchronously, without an exposure, either known, or suspected, in either case—the cases being so situated in respect to social intercourse and roads, that if an accidental exposure to a pre-existing case, were possible in a single instance, the two would be inexplicable; but when to these be added the occurrence of two more within three weeks, alike inexplicable as to their source, it is as clear as a moral axiom, that they are to be regarded as having an ærial origin. Thus introduced, the disease was found to be "catching" to some extent. No individual has contracted the disease by a casual visit to the sick rooms, but of those who were over it through a course of the disease, about *one half* contracted it. The disease then spread in two ways by

infection in the air, and by injection, or by contagion if you prefer, from the persons who were laboring under it. Indeed, a full investigation of the facts in these cases, convinces us that all contagious disorders are kept in existence by an air which elaborates the poison, rather than from poison arising from the bodies of the sick. To this conclusion we incline for the following reasons:

1. These diseases must have originally commenced from some poison in the air or otherwise, *prior to the development of the disease*, and if so, the same condition may recur from time to time, or remain more or less permanently fixed.

2. The rapidity with which such disorders run over countries, shows their atmospheric cause. For example, Scarlatina has been known to spread over several States in the space of two or three months; and small pox in a single season, in 1614, spread over Alexandria, England, Crete, Turkey, Flanders, Poland, Calabria, Italy, Dalmatia, France and Germany. To suppose that this could be done, except in an air which elaborates the poison, is absurd.

3. These diseases have their periods of dormancy and activity. Thus Sydenham found that small pox and dysentery alternated, the latter prevailing from July to December, and the former from December to May following, and so on for a series of years.

4. Doctor Gregory states that a large majority of those who come to the smallpox hospital had not been exposed to that disease.

5. Facts show that persons in *solitary confinement* have contracted the smallpox, thus proving its atmospheric origin.

6. To these may be added the fact that these diseases do not at all seasons spread, except by immediate contagion, no cases occurring which could not be traced to exposure to the sick—though as great facilities were offered for its spreading epidemically, as existed in seasons in which they over ran whole countries.

Influence of Vaccination.

When the disease first commenced we were anxious to ascertain whether vaccination would have any influence over it, and if so, how much. But the first cases having been vaccinated—and one of them but six months before, and this being one of the most violent cases, and finding the next two cases, one of which was also violent, had also been vaccinated, we had little hopes from that

source. Looking through the epidemic we find that, of the *twelve* cases who contracted the disease without exposure, *nine* had been vaccinated. But as vaccination was afterwards practiced, we have not been able to ascertain whether more would contract the disease if left unvaccinated, than otherwise. In some instances, circumstances would seem to warrant the opinion that *it did*, but in others, the *reverse*. In one family of eight persons, *one* of whom had had *small pox, four* had been *vaccinated and three unvaccinated—all had the disease*. In some instances, persons exposed to the disease, though unvaccinated, escaped; but as most persons were vaccinated when the disease first entered the family, we have had narrow means for the establishment of data.

These are briefly the facts in regard to vaccination. One fact in relation to small pox. During the epidemic six persons who had small pox, were exposed to the disease, by attending on the sick, and of these three had the disease. Williams says one in *two hundred* will have small pox a second time. How does this fact stand compared with the opinion that the present epidemic is simply variola. We have heard it said that the greatest orators and logicians have been silenced by the force of a single fact.

As a general rule, it has been set down that one in twenty, who have been vaccinated will contract varioloid, and very rarely indeed true variola, all which so far as it goes. is in favor of the view we have taken of the disease; but we are not disposed to depend on any extraneous evidence as a means of diagnosis. The decision of that question depends on the disease itself.

Besides, be the effect what it might, no certain conclusions, perhaps, could be drawn from that source. If it were shown that vaccination protected persons against any given disease, it would only show that it protected them against *two diseases* instead of one. All are aware that it has been ascertained that vaccination affects favorably other diseases than small pox—for example, opthalmia, bronchitis, hooping cough, &c. It has also been found that at least one poison, *known* to have no relation to small pox, is equally protective against small pox as vaccination. Hence, even if it were admitted that vaccination protects against small pox, *in consequence* of the two diseases being *essentially the same;* still it is seen that identity is not necessary to the *possession of protective powers* against other diseases—therefore, vaccination protecting the system against a disease, does not show that the disease has any relationship either

to cow pox or small pox, and hence is not a legitimate means of diagnosis. Besides, we strongly incline to the opinion that those who have labored to show that cow pox and small pox are the same disease, have as yet failed. It has perhaps been proven, that in a few rare instances, small pox has been reproduced in the cow, horse, dog, sheep, &c., and that the disease thus reproduced has been again established in the human subject—but that natural vaccine or cow pox, and the innoculated or the casual variola vaccine, are one disease, we think still requires proof.

Conclusion.

Wilson remarks that "it is utterly impossible to confound the mature pustules of small pox" with any other disease of the skin, and we believe the remark to be true; and we add that we think it would be equally impossible for any individual to witness the series of cases which we have seen, and endeavored faithfully to describe; and examine them as we have done, through all their stages, and yet mistake them for small pox, or any other affection. It is by no means wonderful, however, that even a respectable physician, with a limited means of observation, should mistake in the diagnosis of a disease having so many *general resemblances* to small pox, as our disease possesses. Many have been deceived by resemblances, mistaking them for *identity*. How close the resemblance of spurious coin to the genuine! How many tolerable judges have been deceived by counterfeit paper! In the trial of Dr. Webster for the murder of Dr. Parkman, unimpeachable witnesses swore that they saw the latter in the streets, when in truth the Doctor was at the time dead! It was a man who resembled him, and if the witnesses had made a closer observation, they would have discovered their mistake.

It has been an interesting question with us, whether this disease has prevailed in other parts of the country. Already we have found a disease *probably identical*, which prevailed in the latter part of the past winter and spring in Erie county, Pa. We received an interesting communication from Dr. Spencer, the physician who attended most of the cases of that malady. We take the liberty to make a few extracts from his letter, and we shall continue the inquiries as respects their identity.

He says. "Here and there a case occurred like true variola, *to to the casual* observer, who had not investigated the case, but who, after a close investigation, could discover a great contrast." * *

The vesicles were elevated above the surface, and did not present that depressed appearance that is so strong a mark of true variola. * * * Vaccination had no influence over it. The worst cases I saw had been thoroughly vaccinated. * * * The contents of the vesicles were much more watery than small pox, and did not have the appearance of pus." Our friend also remarked that "great excitement prevailed for a time" in his neighborhood. It was to be expected. He had discovered something new, and every man who discovers an important truth, or fact in science or art, in neutral or moral philosophy, unless it be so plain that "a fool cannot err therein," must expect, like all his predecessors, to be soundly abused for the discovery. Well do we remember that the pious cotemporaries of Dr. Jenner represented him as "a horrible monster, with the horns of a bull, the head and hoofs of a horse, the jaws of a krakin, the claws of a tiger, the tail of a cow, and all the evils of Pandora's box in his belly," simply for the discovery of a fact which his neighbors did not appreciate! Nothing is more certain, than that in this disease, the unscientific observer, or even the man of science, if he be an *old fogy, dyed in the wool,* will see little else than small pox in disease like this. And in such an emergency the fears of the masses, always electrified at the bare thought of small pox, blind their judgment, and hence the two classes accidentally fall into *harmony;* and the former become oracles, for the time being, for the latter. Then all the false and exaggerated stories of the contagiousness of small pox, which the ignorance and the fears of a score of generations have invented, are raised from the dead, and every man who has in any way approached the sick become walking pestilences, upon whose garments a frightened multitude can see a bristling array of mortally pointed arrows, ever ready to dispense putridity and death! In short, like the British colonels in the days of our fathers, they are prepared to

> "Think horned bugs bullets all through fear,
> Musquetoes take for musketeer."

There is a moral, as well as a physical contagion; and many of us have experienced more or less of the noxious qualities of both. In such circumstances nothing but a *knowledge of the truth,* and a clear perception of *fallacy,* can prevent a contamination.

Such an one is reminded of the lion and the ass who went hunting together. The ass went into the cave and kicked and brayed

and made a mighty fuss, and thus succeeded in frightening the goats. When the ass came out of the cave, he asked the lion if he had not made a noble fight and routed the goats properly. Yes indeed, said the lion, and I assure you, you would have frightened me too, *if* I had not known you *to be an ass.*

But to show that these pedestrian *street professors,* who teach that small pox *is portable* on the garments of *casual visitors,* and even so positively so, as to warrant the unqualified assertion that in an epidemic every case which has not been traced to actual contact with the sick, has resulted from that source, are better "posted up" in superannuated medical hypotheses, popular errors and vulgar conceits, than they are in the medical literature of the day, we will refer the reader to the opinions of a few of the most erudite modern authors on that subject.

Prof. Dunglison, in his practice says, speaking of the small pox virus—"the contagious miasms can attach themselves to clothing, and if air be excluded from the fornites, they may communicate the disease for a long time afterwards, * * there can be no doubt, however, that free ventilation will prevent this; for the author has never known a case in which the practitioner has been the agent of conveying the disease from one house to another."

Dr. Gerhard, in his notes on Tweedie, takes the same ground.

So does Dr. Wood. He says in his practive, speaking of the variolous poison—"it appears to be easily *dissipated by the air,* so as to become *inert,* for well authenticated cases are very few in which physicians have conveyed it from person to person." Being satisfied that this judicious author did not design to be understood by the *rare cases* spoken of, that there was any certain evidence that the disease was ever so conveyed; we addressed him a private note, to which he generously responded, that he "had never met with an instance of the kind personally"—that the case to which he referred, was that of a child who contracted small pox before leaving the room, and hence it was supposed that the physician might have conveyed the disease to him. The author continues—"but as the disease must have been more or less prevalent at the time, it is impossible to say that the child might not have contracted it from some other source." Indeed, it would appear evident that the child did contract it from ærial influence, because as we have seen a person in solitary confinement has taken it, demonstrating that the disease *may be taken in that way*—whereas the instance cannot be found, to *prove the former position,* that we are aware of.

Dr. Condie, the reputable author of invaluable medical works, takes unequivocal ground on this subject. He says—"I do not believe that the physician, or any one, who shall casually visit a patient laboring under small pox, can have his clothes so imbued with the variolous contagion, as to be able to communicate the disease to any unprotected individual with whom he may subsequently come in contact. In the course of a long and extensive practice, including a period during which our city has been visited by several severe epidemics of small pox; I am certain that I have never conveyed the disease to a single individual," &c.

With these quotations we close—we make them hoping that, by drawing the attention of the profession to the subject of the evil consequences of this popular error, our brethren may be induced to prescribe the *pills of truth*, and thus "*open the eyes of the blind*," and then shall error

> "Flee quick away from the pursuing sight,
> Till it is lost in shades, and mingles with the night."

ART. II.—*Cholera, its cause and treatment.* By THOS. W. GORDON, M. D., Georgetown, O.

MR. EDITOR:

I do not design writing an elaborate article. But to comply with a promise made to the lamented Howard, late Editor of the Ohio Med. Surg. Journal. I send you a short paper on epidemic Cholera, with some correspondence in relation to it.

In the paper on Cholera, published in the fifth No. of vol. fifth, there were several typographical errors; in one case, fifty grains was made to read one grain.

Dr. Howard under date, of August 5th, 1853, wrote as follows:

DR. GORDON,

DEAR SIR: I take this opportunity to apologize for failing to make the errata for your article. I was sick during the whole month of July, &c. * * * *

The No. was left in the hands of friends, who did not get hold of your letter of corrections; I intended to have executed the errata as you desired, but I was not able to do it.

After all, I do not think it very essential, as every man would see that there was at least a mistake, and would make the correc-

tions for himself, guided by the tenor of the article, and the size of the doses in other instances. I had a letter from Dr. Hewins of Indiana, who said he was about to write an article for my Journal, on the subject of quinine in Cholera. But on reading yours, and finding your views and experience, exactly in accordance with his, he considered it would be tautology to do so. I urged him to write, as, if the doctrine was true, no matter how much was said about it, the more the better, perhaps he will write yet; can't you write another article? * * * *

Your friend,

R. L. HOWARD.

After receiving the above, I received Dr. Hewins's address, and wrote him on the subject, requesting his experience in the treatment of Cholera with quinine; the subjoined is a part of his reply.

Rockport, Ind., Sept. 20, 1853.

Dr. Gordon,

Dear Sir: Your letter of inquiry is received, and contents noted. In reply, I will here say, my answer must be short just now, as I am much hurried in business. But at a more leisure time, I should be glad to give you a more extended account of my practice in Cholera, and my treatment. Since the receipt of yours, I have been looking for notes, which I made at the time the disease prevailed in our midst, but have not succeeded in finding them, however, I have a note book that never fails me, especially when the facts have been so visibly impressed upon my mind, (I mean my memory.) Dr. Howard informed you correctly, I have used quinine in treating Cholera, and I am forced to look upon it as *the remedy*. I have had an opportunity of testing its virtues for two years in succession, and during the whole time, I used quinine, I lost *not one* patient, while others around me, treated Cholera patients differently, and most of them died, do not understand me as saying, that I think quinine will control this disease in every case, for such seems hardly possible; but in our present state of knowledge of diseases, and remedial agents, I do think quinine far superior to any thing with which we are acquainted. * * * *

I am sir, very

Respectfully, yours, &c.,

L. T. HEWINS.

It is customary with the different religious denominations of the world, to have a *creed* or articles of faith, as a synopsis of their be-

lief, and knowing of no good reason why we may not adopt something of the kind in medicine, or on different medical topics. I hereby send you mine in relation to cholera.

Article 1st. "I believe" that Cholera depends upon a reduction of nervous force in the cerebro spinal system.

Article 2d. "I believe" this reduction depends upon high atmospheric temperatures, or a change from high to low temperature, and possibly *vice versa*, but electric conditions, &c.

Article 3d. "I believe" the derangement in the cerebro-spinal system, produces a similar derangement in organs through its union, with the sympathetic system.

Article 4th. "I believe" intermittent fever depends upon the same cause for its origin.

Article 5th. "I believe" that quinia is capable of arousing to action, the cerebro-spinal system of nerves, and consequently, capable of curing Cholera.

The question will immediately arise; why do you believe thus?

Having noticed the very great resemblance in the two diseases, i. e., the near resemblance of colapse Cholera, to a hard chill in intermittent fever, I immediately tried to think of some cause that might satisfactorily explain the phenomenon. I had seen cases where complete insensibility, had been produced by what I supposed to be the peculiar effect of that nondescript, known as malaria; in which quinia restored the individuals to consciousness and health in a very short time. I had also seen cases of uterine hemorrhage, which I believed to depend upon the same cause, and treated them accordingly with entire success, in the first mentioned cases, i. e., those of spasm or complete loss of sensibility. I attributed it to the impression made directly upon the cerebro spinal system of nerves. That of the second, I believed to depend upon a loss of tone produced the same way, but showing its action upon the uterus, through the union of the sympathetic system with the third and fourth sacral nerves. In these cases, as before stated, I found quinia invariably controlled the hemorrhage.

A case of this kind, I have had under treatment within the last few days, which was controlled without trouble by its use, after resisting the usual remedies.

This woman has been subject to hemorrhage attacks for several years; at one time, she was confined to her bed for six weeks, with

an attack (as she says) similar to the last, and treated as is usual in those cases.

Believing that the resemblance existing between a severe chill in ague—and collapse in Cholera, must have a similar cause to produce the effect; I at once used sulphate of quinia to control the disease, with acetate of morphia to relieve the pain. The disease was controlled in fifteen cases, the only cases that I have had an opportunity of trying it upon.

Will this theory account for the intense gastric irritation?

In intermitting fever, we know there is an oppression of the stomach, and frequently vomiting; why is it? Simply because the impression from the nervous centers is carried along the two pneumogastictric nerves to the stomach itself, and also to the sympathetic of the solar plexus. The nerves of the stomach being large, the effect upon that organ must be great.

Will it account for non-secretion of bile by the liver, or the flow if secreted?

Here, again, the cerebro spinal system, through the pneumogastrics, and especially the left one, gives the supply of nervous power, with the assistance of the sympathetic, through the cœliac plexus, a loss of power, or an increased action beyond the healthy standard in the cerebro spinal system, would consequently destroy or derange the proper function of the liver. Will it account for the non-secretion of urine in Cholera? Here, as in the liver we find that the kindneys are supplied with nerves from the cerebro spinal, and sympathetic systems, and doubtless the effect would be a loss of power to perform their proper functions.

Will this theory account for the transuding of the serum, through the vessels into the intestinal canal?

The splanchnic nerves belong really to the cerebro spinal system, or they at least contain much more of the spinal nerve fibre in their composition, than they do of the sympathetic, as is shown by their firmness and color. The superior mesenteric plexus of nerves is also quite white and firm, showing the spinal nerve filaments in its composition; this plexus supplies the mesenteric artery with nervous power, with the assistance of an union with the right pneumogastric, and the cœliac plexus. Such an union, and appearance proves at once its cerebro spinal preponderance of nervous force. This plexus also supplies the jejunum, ileum, ascending and transverse, colon with nervous power, and furnishes the arteries of the

abdominal viscera with nervous force. Then, if there should be a loss of power in the cerebro spinal system, would not the arteries supplied with nerve power, principly from the spinal fibres loose their power also ? and would not the serum, per consequence, pass rapidly through the debilitated vessels.

Now what is the history of Cholera, and what the temperature where it has raged, as gathered from those who have watched its action closely in the different places where it has prevailed as an epidemic.

The Official Circular of the British Board of Health says, "The climate of St. Petersburg, especially in Spring, is damp and relaxing, and the inhabitants are very subject to typhus fever, diarrhœa, dysentery, and scho:butus," ******. And, in speaking of the symptoms of Cholera, "The disease assumed two distinct forms or *stages*, one of which was characterized by an increased action of the *nervous*, and sometimes of the vascular system, together with great derangement of the digestive organs ; the other by a *prostration* of the *nervous* and *circulating* function ; in a certain proportion of cases the disease passed through both their stages, presenting many varieties, according to the combination and predominance of the symptoms, either of excitement or prostration. In others, the period of excitement was altogether wanting, and that of prostration characterized the whole course of the disease. The morbid principle which acts as the proximate cause of the disease, excited thus in some cases an imperfect reaction, followed by prostration ; whilst in others, it seemed so completely to depress and paralyze the powers of life, that the patient sunk without an effort of the constitution at reaction." Now this description seems to me to agree with the theory that it is an impression made directly upon the nervous centers ; and that when the person atacked is of a peculiarly nervous organization, or the cause is intense, then the patient is stricken down without sufficient power being left in the nervous centers to react in the least, and I think similar to those cases that I have referred to in the beginning of this article, as being prostrated by spasm in one form of intermittent fever : and, again, those where reaction took place, through the effort of the constitution, I believe to be similar to those cases of remittent and intermittent fever most common, though of a higher grade of disease, or one that is more intense in its action upon the animal economy. Again, from the same paper—"The *stage* of excitement was usually ushered in by shivering, giddiness,

and great faintness ****. These symptoms, which were usually in persons of robust constitutions, *after lasting from six to about twenty-four hours, were succeeded by the period of prostration or* COLD STAGE. ****. Thus the whole surface of the body became as cold as marble, and covered sometimes with a clammy moisture, the pulse extremely feeble, and often imperceptible, the face sunk and the features contracted sometimes nearly half their natural size; the eyes sunk deep in their sockets, *and surrounded by a dark circle and the pupils generally dilated,* the *cheeks, hands, feet and nails assume a leaden blue or purplish colour.* *** The disease in a few cases set in suddenly, with great prostration, terminating in death without any reaction, in the course of six or eight hours; *this was its most malignant and appalling form,* well designated by the French, *Cholera foudroyant.* There was sometimes very little vomiting or diarrhœa; in such cases, the prominent feature being prostration and cramp. When reaction took place, either spontaneously or by the aid of remedies; this did not always terminate in restoration to health; but the Cholera was often succeeded by various cases of secondary disease; the most frequent of these was Typhus Fever, *between the congestive state of which, and the force of the morbid derangement in Cholera, there is considerable analogy."—N. Y. Annalist.*

Dr. Garrod says, "In the first place it would appear that the Cholera poison, when introduced into the blood in sufficient quantities, causes an intense exosmotic action of the alimentary canal, at the same time destroying its endosmotic power. The blood, therefore, being deprived of a certain amount of water and salts, by the intestinal evacuations, and not possessing the power of regaining these by absorption from the stomach. ***. But a question now arises, Is this condition of the blood essential? And cannot the *stage of collapse* be induced by the direct influence of the poison? There are certain cases known as *Cholera Sicca,* which would seem to favor this view.—*London Jour. Med.*

Now, does it seem unreasonable to attribute this exosmotic power or action in the intestinal canal to a loss of one in the cerebro-spinal sytem of nerves, and the restoration of the endosmotic power to a restoration of nervous force.

Dr. Bœrstler says, in a letter to a friend, when speaking of collapse Cholera, "It did seem to me that many patients were hurried off by the incessant frictions expending the little remaining nervous power."—*Ohio Med. & Surg. Jour., Vol. 1st, p.* 377. In all re-

ports upon Cholera that I have examined, when any thing is said of the state of the atmosphere, it has been declared to be of a very high temperature at or about the time Cholera had made its appearance in the locality where the report was made. Mr. J. Sullivant, of Columbus, Ohio, says, of the temperature at the time Cholera broke out in that city in 1849, that the 20th and 21st of June, were the hottest days they had had that summer, experiments on two thermometers—a wet and dry bulb, placed in the shade, ranging from 84 to 87 degrees, *Far.* On exposure to the sun, on Wednesday, the mercury rose to 114; and, on covering the bulb with black corded silk, to prevent any reflection of the sun's rays, it reached 126.—NOTE, *Ohio Med. Surg. Jour., Vol. 1st, p.* 577. DR. WATSON, in *his practice*, says, *page* 804, *Condie's Ed.*, 1853—"I have alluded to the influence of *hot weather*, in predisposing the system to be affected by the exciting causes of diarrhœa." Again, on the same page, in speaking of the common Cholera of England, he says, "The Chief cause of cholera, such as has now been described, appears to be casual exposure to cold after a continued high temperature of the atmosphere." He is not, in this place, speaking of Epidemic Cholera, it is true; but is Epidemic Cholera any thing but an increased action of the same cause? that produces common Cholera? Dr. Gayley seems to have used Quinine, sometimes in Cholera; but he did it, he says, giving his own words, "during the convalescence." "With several of these, I tried an acidulated solution of Quinia; but the stomach would not tolerate it.—*Amer. Jour. Med. Science.*"

Dr. C. A. Finley, Surg. U. S. A., at Newport Barracks, Ky., in 1847, says, "The treatment, in every case which has occurred since my special report, has been that laid down in that report." The free exhibition of Calomel and Camphor, and the Quinine after reaction was fairly established."—*O. M. S. Jour. Vol.* 2, *p.* 136.

Now the difference between me and the men above, in using quinine in Cholera, is simply this: I use it to arouse nervous action, and, therefore, to produce reaction, while they would use it incidentally as a tonic to sustain the powers of life, after reaction has taken place. They depend upon Calomel and Camphor in large doses, and I depend upon quinine in full doses, and in collapse in large doses."

Now, are there any forms of disease known as cerebro-spinal affections. That would seem to similate the derangement, seen in the Cholera? Dr. Stone, of Auburn, Mass., in speaking of *cerebro-spinal meningitis*, in the *Bost. Med. & Surg. Jour.* for April, 1849, in

relation to cases reported, "we should repose some confidence in liberal doses of the Sulphate of Quinine, administered immediately on the development of the disease." This would surely be striking out from the track, if the disease was cerebro-spinal meningitis. May it not, however, have been of the same form of disease as that referred to in the beginning of this paper, under the term spasm or complete insensibility?

In all those cases, the individuals were taken with convulsions. Again, Dr. Robert King Stone, of Washington, D. C., in a letter to the editor of the Boston Medical and Surgical Journal, says:—"The cases of which I speak, were three in number, and confined to colored servants. Before being called to the cases, I had heard of *sudden deaths from perfectly inexplicable causes*. Two of the cases which were my own, occurred in colored boys, between 14 and 18 years of age, and the disease commenced with slight chill and bilious vomiting. Unconsciousness, with convulsions, soon supervened, and the head was strongly thrown backwards, with great tenseness and rigidity of the posterior cervical muscles. * * * * The pulse at the wrist was small, the heart's action rapid and feeble, extremities cold and clammy, and when answers could be obtained, great pain complained of at the *scrobiculus cordis* and *nuchae*. At the moment of seeing the first case, which was a very powerful boy of 18, thus attacked whilst in perfect health, hot baths were prescribed, venesection liberally used, and calomel in full doses. The convulsions ceased for a time under this treatment, though the neck remained rigidly fixed, and as there was apparent periodicity in their returns, I resolved to administer large doses of Quinine, in which resolution I was confirmed by my friend Dr. Witherspoon, U. S. A., who was accustomed to congestive malarial diseases. * * * * In spite of the energetic treatment, the convulsions returned with periodicity, and the head seemed to be thrown still farther back at each access. The Quinine was, therefore, solely relied upon, and in the case of the elder boy, I administered one hundred grains of the sulphate with a goodly quantity of capsiccum in twelve hours, whilst in the same period the younger took eighty grains. * * * * It was requisite to revert to the Quinine again on the 2d and 3d mornings, as there was every indication of a return of the alarming symptoms; the quantity required, however, was not more than a dose of fifteen grains on each occasion. * * * * The third case, which I saw with a friend the week afterwards, improved rap-

idly, almost marvellously, under scruple doses of Quinine. * * Several cases were said to have occurred during the same month, and were fatal; in fact two cases were reported to me as having died in the first access. * * * * A few weeks after this, Dr. Witherspoon brought me, from the Surgeon General's Office, U. S. A., an extract from the report of a surgeon stationed at a western post, who had seen an unusual number of congestive cases at the *same period.* "These followed generally an attack of intermittent, when the previous symptoms could be learned from the patient, but too often they were brought into the hospital in a state of insensibility, or convulsions, from which they often never roused. And a very peculiar symptom attended most of these cases; a stiffness of the neck, which caused the head to be thrown back, and the convulsions were decidedly of a tetanic character, with a sickening, personal odor, almost intolerable; in all cases where these symptoms existed, the attack proved fatal. Mortality, nine out of eleven. Treatment—*depletion* and opium. *Bleeding* was *inadmissible*, as generally the patient became at once so prostrated that the pulse was scarcely to be felt." I was unable to find whether Quinine had been used in these cases. It is well understood in our region, however, that it is the only remedy in *malarial congestive* diseases, and must be used at once, and decisively. I would refer to the recent work of Fardiru Pathologie Medicale, Paris, 1843, article "Fiebre Intermittente pernicieuse." He describes almost the same symptoms, and denominates the disease as the meningic or tetanic form of the fever. The treatment is Quinine in large doses. During the last winter, influenza was epidemic, and at the suggestion of some army friends, I was induced to use the same remedy freely.

The administration of fifty grains at the onset, would relieve in a single night the coryza, and unpleasant febrile condition." * *

I should apologize for making so long a quotation, but the matter seemed so apropos to the subject, that I could not well do otherwise. In this letter of Dr. Stone's there are several points brought out that agree fully with the theory laid down by myself in Cholera, the peculiar cerebro spinal action in the convulsions of some of his cases, the pain complained of at the scrobiculus cordis, as in intermitting fever and cholera, the positive proof that quinine will remove the congestion of the vessels, and also the use of quinine in an epidemic that has been frequently noticed to precede or follow Cholera, and evidently dependent on atmospheric changes for its origin, *to wit:* in Influenza.

Now may we not reasonably suppose that the hyperemia, or congestion of the vessels in all these cases, depends upon less of tone in the cerebro spinal system of nerves? In the cases of Cholera that I have treated, I have used from two to ten grains once in ten minutes to once in six or eight hours, often, but not always combined with acetate of morphia from ½ to ¼ of a grain. After the disease is controlled, I generally have administered Pill Hg., ten or fifteen grains, and a Seidletz powder or two—but for the treatment I would refer to my paper on Cholera in the Ohio Medical and Surgical Journal, Vol 5, No. 5.

ART. III. *Reaction, vulgo Fever, from a work on "General Pathology," now in progress.* By Dr. A. DE LESZCZYNSKI.

When a heterologous body has entered the organism, when the latter has sustained any injury, by which the normal proceedings of a part are interrupted, or impeded, then the general organism, by means of its nervous and circulatory apparatus, will endeavor either to assimilate, or to remove the baneful principle out of its jurisdiction, or to repair the sustained damage, or the latter together. The consequence of this endeavor will be "*reaction.*" The *symptoms* of this reaction are called "*fever.*"

Reaction and disease are to me NOT inseperable ideas. We can conceive disease *without* reaction, but no reaction without an abnormal state of the organism, i. e. *an alteration, a change of organic matter.* The fusion of both into one, has been the cause of sad mistakes, of therapeutical intermeddling, and treating "of things that are not." No one will deny the injurious influences of fallacious doctrines, and how many have been promulgated on the subject *fever.* There were made *idiopathic, symptomatic,* and *essential* fevers. There are *intermitting, remitting,* and *continued,* and any quantity of *fevers* of other appellations. Every disease, nearly, was a fever, and nothing but fevers could be seen, and against nothing but *these fevers,* the therapeutical agents were, and partially still are, directed. No wonder, when physiological data, the physiology of the nerves and nervous actions, the influence of irritation upon them and their reaction, the influences they exercise upon the respective organs, were overlooked, that the true nature of a "fever," of its being a number of

symptoms of reaction, was entirely misinterpreted; no wonder when Eberle says:

"Like many other things which are at once obvious to the senses, and concerning the existence of which almost every one can decide, *fever* does not admit of a strictly correct and unobjectional *definition*; since there is not a single symptom which is invariably present, and which can be regarded as absolutely essential to its existence.',

We shall see if this is so. But, if otherwise, let no one suppose I mean to intimate of being the founder of a more rational doctrine; if anything, I mean to be the zealous promulgator only of more rational doctrines. P. Frank, Schoenlein, Burdach, Valentin, Eisenmann, Mueller, Canstatt, Fuchs, and many others, have preceded me in Europe, (although perhaps their proclamations are uttered in different garbs.) In America, S. Gross, some years, ago, said: "there will be a time when the term *fever* will be obsolete." This time is! Shall we drive after a shadow, when we may have reality? Would the surgeon operate with eyes blindfolded? If not, why does not the internal Physician throw off the trammels which schools have fastened upon him. The time has passed when, even a *Hufeland*, could say: "There is only *one* acute disease,—it is fever."

Organic life consists in an action of an external irritant upon the irritability of an organism. (Irritation and reflex.) It can exist only as long as there exist:

1. *Organic attraction* or organic magnetism, that is, the power of the different structures, in virtue of which they attract only those elements, which are requisite to their individual existence.

2. *Organic equilibrium*, that power, in virtue of which the individual structures, as well as the organism as a unite, expend in proportion to restitution.

3. *Organic periodicity*, that power, in virtue of which the individual structures, as well as the entire organism and certain processes, stand in relation to each other, as regards time. This power might also be called, "*organic symetry.*"

Every irritation will call forth a change in the organic matter, as well, as a change in the organic functions. An inadequate, that is, a too long continued, or too violent irritation, will cause a too great expense of organic matter, and hence functional disturbance, a disturbance of *organic equilibrium*. Reaction, reflex-action, to react against extraneous powers, entering the organism, or against those

impressions caused by matters originated within, but which, on account of their nature can not be tolerated by the organism, is an innate principle, of the *nervous centres*, which to explain, we will succeed as much, as to explain the nature of electricity, light or elasticity.

By reaction, in its pathological sense, we understood particularly a reflex-action, in which *the entire organism* participates, although the irritation, originally, may have been local. It will be greatest when parts indispensable to the normal condition of the functions have been affected, or such as are of the greatest vital moment. The prime movers in *this*, like in *healthy reaction*, are the nerves. Its perceptible symptoms are called "fever."

Any matter causing abnormal irritation, and which therefore becomes hurtful to the organism, we named in another place, "*a noxiousness*. This, it will be seen, is as relative, as that of "poison."

We have seen that a noxiousness may become hurtful to the organism:

1. By its nature, quantitatively and qualitatively.
2. By its location.
3. By attacking a peculiar individuality, and, that these not only will cause the *force*, but also the *degree* of *disease*. The *degree* of *reaction* will be corresponding with the degree of disease, provided the nature of the noxiousness has not been such, as to revoke the three fundamental laws of organic life, or to exclude their re-establishment.

The position which Eberle, and with him the old school, takes in his description of the general character and etiology of fever, I can not entirely uphold. He says:

"Fever consists in an irritated excitement of the *sanguiferous* system, and is essentially connected with a deranged state of the vital properties of this system.

This *irritated* excitement may be the result of irritating causes acting immediately upon the internal surface of the sanguiferous system; or of irritating impressions conveyed sympathetically to *this* system, from a primary focus of irritation."

When Eberle, and others, upheld this theory, they undoubtedly overlooked the nervous influence; they mistook the *result* for the *cause*. The *chief symptom* of reaction, it is true, is an increased vascular action, and in specie, an *increased action of the heart*. This increased activity, as indeed all action, or motion within the body, is

dependent *entirely* upon the *nervous system.* (We remind on the influence of the n. n. vagi upon the heart.)

The second position we find more based upon neurologia, than the former. But when a cause is acting immediately upon the internal surface of the sanguiferous system, or, in other words, upon the nerves of the coates of the blood vessels, then the latter and the *heart* will not be induced to an increased action before the reigning central nervous organ is in rapport with the seat of the abnormal increased irritation. (The proof of this will be found in the chapter on the nervous system.)

Re-action can be two-fold, *reflected* and *central.* By reflected, or reflective reaction, (fever,) we understand that process of the organism, in virtue of which a disturbance, that is, any abnormal *irritation* is communicated by *the conducting nerves* (cerebral, spinal, or sympathetic,) to the nerves of motion of the heart and muscles of inspiration. This transmission takes place within the central organ of this system, (the superior part of the spinal.) *Reflective reaction,* as indeed all acute local disturbances, is preceded, in most cases, by local symptoms, as pain, functional disturbances, &c., which frequently disappear sooner, than the pathological process has spent itself. For instance, in inflammation we see that the pain ceases after exsudation has taken place.

Central reaction is caused by an abnormal irritation of the *central organs of motion* of the heart and vssels, (*superior part of the spine and ganglia.*) It may be caused by purely local, as well as general causes. Central reaction generally *precedes* local spmptoms. An instance is furnished by the different diseases of the blood. By those of them which become local, we observe the symptoms of reaction previous to the symptoms of disease, as in pneumonia, caused by hyperinosis. Here we first observe *re-action,* afterwards local symptoms, whilst in pneumania, proceeding from *local causes,* our observations speak for the opposite.

Prof. C. E. Bock adds to this: "Peripheric re-ation," that is, re-action caused by direct irritation of the motory nerves of the heart, *in their continuation.* But when we reflect upon the fundamental laws of nervous communication, of conduct, and upon the construction of the nerves, upon the multitudinous distribution of their fibres, then we cannot help but see in this, nothing more nor less, but *reflex re-action.* And this we shall not revoke, although we know that acute diseases of the blood will cause the most violent re-

action, whilst purely local diseases are followed only by feeble, and soon passing symptoms of re-action. My creed is, that we have no right to presume a different mode of action in the sympathetic nervous system from any of the others, but that the law of *incidence* and *reflex* here, is qute as binding as there. The violence of re-action in this case, I should rather seek in the attack of the elements of vegetative life.

The character, that is, the *form* of re-action may be double, depending either upon the nature of the noxiousness, or the individuality attacked. It is either:

1. *Simple, benign* re-action, *Erethismus*, or
2. Stormy, ardent re-action, *Synocha*.

Many writers add to this a third form, "*the torpid re-action.*" But these same writers affirm that this *torpid re-action* is caused by a supremacy of the noxiousness over the powers of regeneration, in consequence of which the latter cannot re-act. What is the essence of all this? They say there is a re-action, which, on account of the *suppression* of the *power of regeneration*, does not re-act, ergo: *is passive*. A wonderful explanation! The reason of this said mistake lies, undoubted, in the fact, that these gentlemen compounded *disease* with *re-action*. The form of disease is two-fold; that of re-action only double. As we shall treat of *torsor* separately, we shall now speak of the objectively perceivable form, the image of re-action in general, afterwards of simple and the ardent re-action, in particular.

Symptoms of re-action.—Increased irritability—affection of the sensorium—increased action of the organs of circulation, therefore frequency of respiration abnormally increased contractions of the heart increase of velocity of the circulation, (therefore frequent pulse,) and increased temperature, suppression of the se- and excretions, turgescence and dryness of the epidermis and mucous membranes, thirst. This is the general aspect of every re-action, (fever.) We shall now compare the degrees of re-action:

ERETHISMUS.	SYNOCHA.
PULSE.	
Full, frequenter, easily compressible, neither empty nor hard.	Full, very frequent, hard, tenter, (in some cases small, thread-like,) and easily compressible; pulsation of arteries, as those of the temporal and carotids, visible.

EXPRESSION OF FEATURES.	
Composed.	Painful, restless, eyes sparkling, blood-shot, vessels of the conjunctivę bulbi oculi et palpebrarum conjested.
SENSORIUM.	
Slightly abnormal.	Strongly attacked, commences with violent chills, violent headache, strongest in the occipital region, restlesness, insomnia, delirium, strongly, only painfully increased sensitive feelings of the organs of sense.
REMISSIONS.	
Every afternon very perceptible.	Hardly any.
SKIN.	
Not very dry.	Very dry and turgescing.
TEMPERATURE.	
Nearly normal.	Highly increased, subjectively and objectively, expiration very hot.
TONGUE.	
Moist, slightly furred.	Changeable, very red, sometimes covered with a whitish fur, inclined to dryness.
THIRST.	
Not abnormally increased.	Violent, insatiable.
URINE.	
Slightly reddened, quantity as usual, or nearly so.	Dark-red, causing pain in the urethra when passing, contains much urea, deposits a precipitate when standing, scanty.
ALVINE EVACUATIONS.	
Not entirely suppressed.	Suppressed, hard, of dark color.

Rythmus of reaction.—By rythm, we understand a certain order in which certain actions follow each other. By rythm of pulsation, for instance, we understand that order in which one contraction of the heart follows its predecessor.

Reaction, (the increased action of the power of regeneration against a noxiousness,) is subjected, like in health, to daily rythmical oscillations, and becomes perceptible to us, by an increase or abatement of the enumerated symptoms. A strong guide is afforded by a more slow or rapid circulation, at different times of the day.) Whenever individual life, or as Schoenlien calls it, "the egotistic principle," is stronger than the noxiousness, reaction will be strongest *in those periods* in which the power of regeneration, *during the normal state,* is in ascendancy, or, in other words, *is most active.* Upon this law *Constatt* rested, when he said, "We have, therefore, a right to be convinced that NOT *the increasing noxiousness, but the* PERIODICALLY INCREASING RE-ACTION, *causes the daily exacerbations, which particularly return nocturnally.*" Reaction becomes successful in consequence of these exacerbations. The stronger the latter are divided from the remissions, the surer will be the success of the power of regeneration. We meet strongly marked rythm mostly in simple, etheric reaction, wherefore the latter has received the name of "*Remitting Fever.*" In open contrast with this, stands the *Continued Rythm.* Reaction may have been too strong from the very onset, or it may have been simple; but from various causes, as from violence of the noxiousness, or even mal-practice, it is changed into synocha. From this reason, the *ardent reaction* has been named, "Contingent Fever."

Prognosis.—The etheric form of reaction, generally ends good, when not mistaken, and when more left to itself. But when the power of regeneration, by violent interference, or other causes, becomes weakened, then it will change into *torpor.* Again, if the causes of irritation should either increase, or not be removed, it will change into *Synocha.* When this latter is not too violent, and by proper interference of art, it may be led back to the first form.

Should we not succeed in this, then the power of regeneration will be exhausted; the patient dies either during its acme, or, at the best, it will change, like the former, into *torpor.*

It will be necessary for the practitioner to know that a patient has fever;" but this knowledge is by no means *as important,* as we are told. It is true, the symptoms of reaction will call our attention to

the existence of a disturbance on the organism; but they tell us neither the nature, or the seat of it. Moreover, they frequently, if *only* guided by them, will lead us astray, either exaggerating or diminishing the malady, when, perhaps, in the first instance, we ought to *wait;* in the second *act,* and that *quickly.*

We may observe a great many curious and alarming phenomena, during what is generally termed "afever," caused by the motory and sensitive parts, of the cerebral, spinal and sympathetic nervous systems; as spasms, anomalies of se- and excretions, pain, etc.; we may observe a number of disturbances of the functions of the central nervous organs, as divers sensations, abnormal perceptions, etc. Therefore, it must be of the greatest import to the physician to distinguish between *fever* and *disease,* between cause and effect. True enough, it is a great deal easier (when attending a patient) to call reaction a disease, and only associate with it a few secondary symptoms, *which we just chance to see,* as pain, or cerebral symptoms, and call it a *rheumatic fever,* or a *nervous typhoid fever,* than to find the cause which produce these symptoms. *Sapientia sat!*

COLUMBUS, O., FEBRUARY, 1854.

ARTICLE IV.—*Hospital Reports.* BY J. G. CHAPIN, *of New York Hospital.*

MR. EDITOR: I send you an abstract of some cases of Albuminuria, with microscopic notes of appearances of urine and the treatment pursued.

A German, aged 21, a printer, admitted May 7th, during the attendance of Dr. Swett. The friends of the patient stated that, for the past four days he had been delirious, and most of the time insensible; and that, in addition, there had been almost entire suppression of urine—the past 24 hours having voided 3 oz. It was also stated that he had performed his usual work on the 4th, and this attack appeared early and suddenly the following morning (5th.) On the evening of admission, there was delirium and partial coma—could be aroused, and answered intelligently, but slow and indifferently; the breathing was oppressed, the inspiration long, deep and

sighing. The pupils were natural, but *vision* imperfect—unable to distinguish the small letters of a newspaper. *The skin was dry*, and natural in temperature. The pulse was 70. There was a dropsical accumulation about the legs, abdomen, face and hands. Prognosis bad. Was directed to receive externally the hot vapor bath, and there was administered internally a mixture of Spts. Minderi, Spts. Nit. Dulc. & Ipecac. every 3 hours during the night. Cupped on the lumbar region.

This treatment was steadily pursued, and the following morning there was a marked relief of the urgent symptoms. The urine was freely discharged during the day to the amount of one pint, which, on examination was found of Sp. Gr. 1010 faintly acid, almost entirely coagulating by nitric acid, and less so by heat.

The Microscope showed, 1. Crystals of uric acid. 2. Fibrinous casts of uninary tubes containing free fat globules and epithelial cells loaded with oil globules—three free cells containing oil.

May 9.—Patient had recovered sufficiently to give a history of his case, and stated that three months ago, he first noticed an ædematous swelling of his feet, afterwards extending as detailed above—had not been confined to his bed, and attributes his present attack to exposure at night after drinking freely of wine. The quantity of urine, the last 24 hours, has amounted to 3 pints—the skin is *moist*—the vision is restored.

May 25. The diaphoretic treatment has been continued. The quantity of urine, the past week, has been, in pints, 6, 7, 7, 8, 5, 8, 5, of Sp. Gr. 1008. The bowels have become, spontaneously, very much relaxed. The dropsy has been very much relieved.

To-day patient suffered a relapse, attended with dimness of vision, diminished quantity of urine, and increase of dropsy. The treatment was persevered in and continued until his discharge, August 10, with the exception of the month of July, during which he contracted dysentery, and was treated for it. On his departure from the Hospital, there was remaining slight œdema of his feet and face. His urine was examined; it then contained albumen; Sp. Gr. 1014. The microscope showed epithelial cells, free oil globules, and cells containing oil.

Case II.—An unknown man brought to the Hospital in April, 1852. The people who brought him found him in the street. He was comatose, pulseless, and in a moribund condition. He could be roused with difficulty, but gave no account of himself. There

was some œdema of feet, and his urine drawn from the bladder by a catheter, yielded abundance of albumen. It was about 3 oz. in quantity.

He was treated with diaphoretics and hot air externally; but reaction failed to ensue, and he died the next morning.

The microscope showed the presence of fat globules in epithelial cells. The same was noticed, together with free fat globules, on examining the kidneys after death.

CASE III.—A German, aged 35, a hotel keeper, was admitted Sept. 19, during the attendance of Dr. Bulkley. Patient stated that he had suffered several weeks from obstinate constipation, and neuralgia, pains of legs, induced from exposure to the fumes of a freshly painted room. The characteristic blue line was developed along the gums. He had been directed by his physician to use inunctions of turpentine, which he had steadily persevered in. The day of his admission he began to complain of pain in the head—of drowsiness—dimness of vision—which continued till his admission—was not able to distinguish any object in the ward, and soon not even light. With this there was suppression of urine; the little fluid voided seemed mostly blood. He was cupped over the region of the loins—a sinapision directed. Internally he received spts. wormwood and ipecac, and externally the vapor bath.

On the 20th, the comatose state was well marked, attended at times with a low delirium. The amaueosis continued.

Toward evening the skin began to act freely, and on the 21st there was a bountiful secretion of bloody urine. The vision on the 22d was quite restored. The patient was discharged on the 29th, cured.

This statement is an abstract of the case. The hematuria and suppression of urine seemed clearly traceable to the turpentine.

I will briefly recapitulate the points of interest in the above cases, and I wish to say that the observations as reported have repeatedly been borne out in all the cases of Bright disease the past year in the Hospital.

As will have been observed, the first reported was discharged from the Hospital relieved; the second died; the third possesses much interest in a practical way, though not belonging to the disease under consideration.

The first had enjoyed very fair health till the sudden exposure induced the very critical state in which he was admitted—which was probably the case with the second, from the fact of his sudden insensibility in the street.

On the admission of the three, the symptoms were urgent, and the prognosis extremely unfavorable; the first and third suffered from a complication rarely met with in this disease—amaurosis—depending on the poison from retained urea in the blood. The prompt administration of diaphoretic remedies relieved the first and third patient, but failed in the second.

The microscope showed in the urine the presence of epithelial cells, and of fibrinous casts. The cells contained oil globules, and the casts were filled with cells; there were also free oil globules from cells which were gorged and bursting.

Post mortem examination showed in case second that the kidneys were larger than natural, firm, of pale matted appearance, presenting abundant evidences of fat in the cortical and tubular structure, and in addition was found in the muscular substance of the heart. Probably if the examination had been extended further, it would have been found in the liver and tissues generally.

If any extended observation could establish the fact of its general diffusion throughout the tissues, it ought to go far to show the existence of a great constitutional diathesis, in which there was this deposit, owing either to defective calorification, or to a depraved assimilation of the elements of nutrition. The kidneys take the function of eliminating this matter, and may, long before the presence of albumen, successfully be equal to resist the establishment of this diathesis, if we may infer from the occasional disappearance of the fat, which is observed often in the course of this disease. It is said to be true that a certain amount of fat may exist normally in the urine without evidence of disease; but how little and not constitute a disease, we do not know, but we do know it is expelled at the expense of the glandular epithelium, and its continuance at least should be a source of solicitude with the patient as to its source.

The glandular epithelium, then, may be destroyed, leaving the basement membrane denuded, allowing the transudation of the seruous portion of the blood, explaining the continual presence of albumen long after the apparent restoration of the health of the patient; then, again, the part long furnishing the albumen, its own function destroyed from disease, may be absorbed, and a healthy urine be again present; hence by absorption, successive uriniferous tubes, in cases of long duration, we may see a kidney diminished one-half its size. As leading to this result must the continual presence of the cell be suspicious.

The diagnosis of this fatty degeneration, and especially the prognosis, must rest principally with the microscope. The albumen is only a sign, but does not *satisfy* any more than the emaciation of phthisis, though both are present always, either of itself may be fatal to the patient. It comes in the disease to signify often the injury is irreparable—the sign of a great constitutional malady, it manifests itself not *in*, but *at* the kidneys secondarily. But it may in its course develop itself variously, few cases of fatty kidney completing their course without some serious complication.

As a matter of interest, I have collected the statistics of all the cases treated as Albuminuria, including of course those following scarlatina, from the case-books of the hospital. They amount to 77 the past 11 years.

Of this number 32 died, 22 were discharged relieved, or at their own request. Of the 77, the complications occurred in the following order of frequency, though it must be observed that several suffered with a number of affections at the same time :

10 with hypertrophy and valv. disease of heart.
5 with dysentery.
4 with amarousis.
3 with phthisis.
2 with chronic rheumatism.
2 with hydro thorax.
2 with emphysema.
2 with chronic diarrhœa.

39 were free from complication. Of this number 16 died. Of 38 with complications, 16 died, 22 discharged at their own request, or relieved.

Taking in consideration the fact that the result of many cases was unknown, the average duration of the disease was four months and fifteen days, though in many extending over a period of three and five years. The average age war 35; 62 were males—15 females.

The relation between the presence of albuminous urine attended with dropsy, and a peculiar state of the skin, was pointed out by Dr. Osborne, in the Dublin Journal of Med. Science in 1834. The perspiration in many cases was either extinct, or breaking out occasionally on the face and hands. The skin was dry and shining. Dr. O. supposed that suppression of perspiration was the most common

cause of albuminous urine, and stated the following result—"that whenever general perspiration came on, either spontaneously, or by medicinal agents, the case terminated favorably. He proposed that the treatment should be directed so as to secure the healthy function of the skin—to treat in fact all dropsies, independently of the part involved. Of 36 patients treated by him on this plan, three quarters recovered.

Diuretics have been fairly tried, and at this day are constantly being administered. Of 80 patients treated by these measures by Dr. Gregory, 40 died. If the pathology detailed above be the true one, it stands to reason the mortality might be greater. The cell vitality already below par, could not be expected to undergo increased tension, If we ever could infer the propriety of their administration, it would be in those cases where the dropsy proved obstinate, from the blocking up of the uriniferous tubes, which an increase of secretion would wash away. This would always be a delicate point to decide.

The slow destruction of the function of the kidney bears then an important relation to that of the skin, for upon it must depend the elimination of the excrementitious matter of the blood. The skin will assume this office, and if we may infer from the case first detailed, maintain it a long time.

It is liable to sudden suspension, and death may ensue in a very short time, if it is not restored. Most of the cases of sudden coma from this cause have resulted from excess and exposure. Among that class of persons also exposed to the constant changes and extremes of heat and cold, and to the intemperate if happens the mortality is very much greater. Of the 77 patients 26 were seamen and 11 died. The remainder pursued avocations exposing them to out of door weather. It does not follow the disease is peculiar to this class—they the oftener and sooner fall victims.

I have briefly endeavored to illustrate the following points:

I. Microscopical examinations of the urine confirm the view entertained by Dr. Leo Johnson, that true Brights disease is a fatty degeneration of the secreting cells of the kidney.

II. The important connection this disease has with the function of the skin.

III. The importance of resorting to the most prompt diaphoretic means in sudden suppression of the urine. J. B. C.

PART SECOND.

AMERICAN AND FOREIGN INTELLIGENCE.

Art. I.—*On the Chemistry of Digestion*—By Dr. G. E. Day.

[Our readers can hardly fail to appreciate the following elaborate review of Professors Bidder and Schmidt's work on "The Digestive Fluids and Metamorphosis of Tissue," and of Dr. Carpenter's "Food and the Digestive Process," by Dr. G. E. Day, the esteemed professor of medicine at St. Andrew's. Of the different secretions employed in the digestive process, that of the salivary glands comes naturally to be considered first. The principal use of this would appear to be to promote the conversion of the *amylacea* into dextrine, sugar, and lactic acid, and thus promote the absorption of this class of foods.]

The first point we shall notice is the time required for the metamorphosis of starch by human saliva (the mixed secretion.) If we take a fresh decoction of starch, prepared with distilled water, and proved by Trommer's test to be free from sugar, and if we mix it with an equal quantity of fresh saliva, and agitate the mixture, it will instantly lose its viscid character, and become thin and watery; on testing a small quantity of it for starch, we find the iodine no longer induces the well-known reaction, while, on the other hand, the rapid reduction of oxide of copper (in Trommer's test) affords indisputable evidence of the presence of sugar.

The almost instantaneous induction of this action is a point which must not be overlooked, in considering the question whether this is a special property of the saliva, or whether it is shared by other animal fluids. There can be no doubt, as we shall presently show, that in this respect the pancreatic and the intestinal juices exactly coincide with the saliva; but when we find stress laid upon the circumstance that many organic substances—as, for instance, nasal mucous, pieces of kidney, putrefying serum, &c.—produce similar changes in eight or twelve hours, at 100 deg., or upwards, we may

recollect that at such a temperature, and after so long an interval, changes may be spontaneously set up in a solution of starch. There are, however, a number of animal substances which occasion the appearance of sugar in a solution of starch, in so short a period as altogether to exclude, in such cases, the suspicion of spontaneous metamorphosis; but the action induced by the saliva is incomparably more rapid even than that of any of these substances.

[We may ask, are the secretions of the three sets of salivary glands of equal importance in producing the final result?]

The following are the two principal conclusions at which Bidder and Schmidt have arrived in connection with these points:

1. They agree with Bernard in regarding the parotids as *glandes aquipares;* in short, as yielding a secretion which is unquestionably intended to moisten and saturate the dry food, but whose principal object is connected with the general metamorphosis of the fluids within the body, and which is devoid of any marked action of starch.

2. By the union of the submaxillary secretion, and that of the buccal mucous membrane, there is formed that peculiar ferment which almost instantaneously converts starch into sugar. This active principle is not contained in the cells or other solid particles suspended in the saliva; for the filtered fluid exhibits an undiminished force; and, indeed, this property is not destroyed when, by the addition of a little alcohol, we precipitate the mucous and (entangled in it) these solid particles.

There is still considerable obscurity and diversity of opinion regarding the question, whether the action of the saliva on starch is continued in the stomach. Jacubowitsch, Frerichs, and Bence Jones believe the metaphoric process is continued in the stomach, and Lehmann and Carpenter support this view, while the experiments of Bidder and Schmidt lead to an opposite result.

[It would appear from these experiments that starch should be rapidly converted into sugar in the stomach; but in animals, where it has been introduced artificially, the result has afforded a conclusive demonstration that no conversion into sugar takes place in this organ.]

Now, seeing, in the first place, that the formation of sugar commences in the mouth immediately upon the starch coming in contact with the saliva, and further bearing in mind that no sugar can be usually detected in the stomach, it follows that one of two things must occur—either that the newly-formed sugar is at once absorbed,

or that it is converted into lactic acid, and in this form mixes with and is lost in the gastric juice. In the latter case, we must ascribe to the gastric mucous membrane the power of instantaneously converting sugar into lactic acid—a power which, as we learn by experiments, is possessed neither by the stomachs of herbivora or carnivora; in the former case, we can only say, that so rapid an absorption is perhaps the least improbable mode of explanation.

We are bound to remark, that the above opinion of Bidder and Schmidt, regarding the absence of sugar in the gastric contents of animals fed upon starch, has not met with universal acceptation. It is true that it is in accordance with views previously expressed by Blondlot, and by Bouchardat and Sandras, but it is opposed to the results obtained by Frerichs (who "in at least fifty experiments" constantly found sugar in the filtered contents of the stomachs of men, carniverous and omniverous mammalia, and birds,) and to those of Jacubowitsch.

The secretion of the *glandular apparatus* imbedded in and forming the chief bulk of the mucous coat of the *stomach*, next claims our attention. Many of our best recent observers (amongst whom we may especially mention Hubbenet, a pupil of Bidder and Schmidt's) believe that two perfectly distinct secretions are found in the stomach—namely, the true *gastric juice* and *gastric mucous*. Almost all observers since Beaumont, have agreed in describing the former as a clear aqueous or only slightly viscid fluid, with a strong acid reaction, and devoid of morphological elements—that is to say, of *essential* morphological elements; for on a microscopic examination, a few solid particles, consisting partly of unchanged cells of the gastric glands, partly of the nuclei of these cells, and partly of fine molecular matter, which is produced by the disintegration of these elements, may usually be observed; it is only secreted during the period when digestion is actually going on, and it is this fluid alone which possesses any true digestive power. All that is known of the latter (the mucous) is, that it accumulates in the empty stomach, that it is neutral (unless when slightly alkaline from the commixture of saliva,) and that it exerts no solvent action on the protein compounds. Frerichs, however, whose views on the Chemistry of Digestion are entitled to the highest respect, holds a different view, and maintains that at the commencement of each act of digestion, the round cells in the interior of the glands escape in excessive quantities, and form a stratum of about a line in thickness, which has hitherto been regard-

ed as mucous, and which either invests the interior of the stomach, or surrounds the contents in the form of a white membrane, the latter being especially the case when dry food is taken. The gastric cells become gradually disintegrated during the continuance of the digestive process, and thus afford a continuous source of pepsin or ferment. The digestive act being accomplished, the gastric glands collapse, and in that state no nuclei or cells, and only a few scattered granules, escape from them. During abstinence from food, the morphological elements are again perfectly formed, and the tubes become filled with cells, which probably in very prolonged fasting again become disintegrated.

There are two points in connexion with the secretions of the gastric mucous membrane to which Kölliker has directed special attention, and which we will now lay in a condensed form before our readers.

The first is, that *in many of the mammalia the gastric mucous membrane is covered with a more or less thick coat of mucous during digestion.* This fact was originally noticed by Eberle in the stomachs of rabbits, and he clearly regarded the mucous as essential to digestion.

[The gastric mucus can hardly be regarded as essential to the digestive process, except that it may possibly exert a peculiar influence in certain parts of the stomach. The gastric juice is acid, unless there be an excess of mucus, when it may be neutral, or when a large quantity of saliva is swallowed, when it may be alkaline. It contains a free acid, a highly nitrogenized matter, termed "pepsin," or, more recently styled, "ferment substance." It is perfectly astonishing how enormous have been the labours of these indefatigable labourers, M. M. Bidder and Schmidt. They have recorded no less than 45 series of experimental inquiries, embracing 140 individual experiments.]

Their experiments lead to the following conclusions:

The admixture of saliva with the gastric juice is of no service, in so far as the solution of albuminous bodies is concerned. It appears, that in experiments with gastric juice containing saliva, the number expressing the dissolved per centage of dry albumen was not so high as in corresponding experiments (both sets of experiments being made within the stomachs of dogs) when the saliva was excluded. Whilst in the latter the averages of nine experiments of two hours', nine of four hours', and nine of six hours' duration were 29, 62, and 76 per cent., in the former they were only 26, 45, and 65 per cent.

This difference is unquestionably dependent upon the partial neutralization of the free acid of the gastric juice by the saliva.

Both classes of experiments agree in showing that the acid gastric juice possesses a specific and peculiar power of dissolving coagulated albumen; and if this solution proceeds with more rapidity in the stomach than externally to the organism, the reason is, that in the former the albumen is kept in constant motion, and is being always brought into contact with fresh quantities of gastric juice. Several of Bidder and Schmidt's experiments show, that provided a sufficient quantity of pepsin be present, the digestive power of the gastric juice varies in a direct ratio with the amount of free acid that it contains. That the solvent power of the gastric juice is not solely due to the presence of hydro-chloric acid is, however, evident from the results of several experiments, which showed that this acid, in a diluted state, could only, at the most, exert one-fourth part of the solvent action on coagulated albumen which was exerted by the gastric juice itself. The neutral or alkaline gastric mucous of fasting animals dissolved only very slight quantities of albumen, but on adding hydrochloric acid, the activity of the fluid was at once augmented. On neutralizing the free acid of the gastric juice with potash, its digestive power is destroyed.

With regard to the effects of filtration, they observe that repeated experiments have convinced them that gastric juice from which all undissolved organic matters have been separated acts with the same efficiency as that which has not been filtered. This is obviously at variance with the view held by Frerichs, that new ferment continues to be developed from the cells contained in the gastric juice by the action of the free acid.

The experiments on concentrated and diluted gastric juice led to no definite result; indeed, no experiments seem to have been made with the concentrated fluid till it had been re-diluted with water.

It was proved by several experiments that the solvent action of the gastric juice is destroyed by rapid ebullition, or by evaporation at a boiling heat—an additional illustration of the well-known fact, that ferments are decomposed at an elevated temperature. This may possibly be the basis of the universally recognized dietetic rule, which declares all very hot foods or drinks to be injurious. On the other hand, the solidification of the gastric juice by artificial cold did not destroy its solvent powers—a circumstance which may be consolatory to those who are addicted to ices.

It was distinctly shown by three experiments, that the admixture of bile with the gastric juice—even when the quantity is so small that the mixture retains a distinctly acid reaction—completely destroys the solvent power of the latter secretion upon coagulated albumen.

The quantity of the gastric juice secreted in twenty-four hours was determined by Bidder and Schmidt, in the case of dogs, to be about one-tenth of the whole weight of the body. Assuming the same ratio to be true for men, a man of ordinary weight (say from ten to twelve stone) would daily secrete no less than from *fourteen to about seventeen pounds of this fluid.* This is a very different estimate from that formed by Lehmann, whose calculations were based on the following data:

100 grammes of the fresh gastric juice of a dog cannot, according to Lehmann's experiments, effect the solution of more than five grammes of coagulated albumen (calculated as dry.)

An adult man receives into the stomach about 100 grammes of dry albuminous matter in twenty-four hours.

Hence, to digest this quantity, there must be secreted 2,000 grammes, or four pounds of gastric juice.

We now proceed to the consideration of the *bile*—a subject to which Bidder and Schmidt have devoted much experimental labor. The first question they consider is the following:—Is the bile to be regarded simply as an excretion? This point they have attempted to decide by observations on dogs in whom biliary fistulæ had been established.

A dog on whom this operation was performed, lost two-fifths of its weight in thirty-four days, although its appetite was increased rather than diminished. The emaciation was especially marked both in the loss of fat and muscle. The hair fell off at many spots, or was loosened by the slightest touch. Digestion appeared to go on in an undisturbed manner, although the animal was in the habit of licking the bile as it escaped. The excrements had a greyish-yellow, and sometimes a partially green color; but during the last few days of life, regained their normal color and odor. There were constant eructations of a bad odor, and the breath likewise had a very unpleasant smell. The urine was occasionally so concentrated, that its specific gravity was 1·050. The animal at length (on the thirty-fourth day) died from general exhaustion. Dissection showed that a minute new ductus choledochus had become formed, probably be-

tween the twenty-first and twenty-fifth day after the operation, at which time the fistulous opening presented a tendency to contract. The right half of the stomach presented numerous ecchymoses, both of older and of more recent standing, although there were no symptoms of disturbed gastric digestion during life—that is to say, there was no vomiting, and the appetite continued unimpaired. The rest of the intestinal tract presented nothing abnormal, except that it was of a somewhat deeper red tint than usual.

A second dog, similarly treated, did not present a voracious appetite, or exhibit the same amount of flatulence, or such a tendency to putrefactive decomposition as the former animal; but in the course of twenty-seven days (when it died) it had lost about one-half of its weight. Except that the fat and the muscles had almost disappeared, none of the organs were found to present any marked peculiarity.

Since the quantity of bile that escaped from the system in these two cases was not compensated for by the additional food that was taken, the idea suggested itself that the emaciation and the disturbed condition of the metamorphosis going on within the tissues might be due to the continuous drain upon the system. This view was confirmed by the two following experiments, in which this disturbing cause was eliminated:

A dog, whose spleen had been extirpated, and in whom a biliary fistula had been established fourteen days afterwards, took such large quantities of food as more than to compensate for what was lost by the escape of bile. Its bodily weight, under these circumstances, did not materially diminish during the space of two months, at the end of which it was killed. The normal development and power of the muscles were retained, but the fat had gradually disappeared. Putrefactive decomposition of the excrements, and abundant flatus of a disgusting odor, were likewise observed in this case.

A very emaciated dog, whose spleen had not been extirpated, and in whom a biliary fistula had been established, regained its healthy appearance and its previous normal weight nineteen days after the operation. During this period it took more meat and bread than were requisite for it in its perfectly healthy condition. The ductus choledochus subsequently became re-established, and the weight then again materially increased.

From these experiments, it seems clear that the question which they attempt to solve cannot be directly and positively answered ei-

ther in the affirmative or negative. By an artificial interference with the organism (by the establishment of a biliary fistula) the bile may be reduced, in a certain sense, to the condition of an excretion, if the loss thus occasioned to the system at large can be suitably covered. If this compensation be impossible, the derivation of the bile from the intestinal canal and its direct external discharge in a comparatively short time, induce fatal emaciation and debility. For the full comprehension of the question in all its bearings, we should know the quantity of bile secreted in a given time, and the average amount of solid constituents which it contains, the share which it takes in the process of digestion, the changes which it undergoes during and after its resorption into the blood, and lastly, the form in which it is finally eliminated from the organism.

In the strict sense of the word, we can hardly regard the bile as a digestive fluid. There is no special class of substances whose solution is dependent upon it. If, as certain experiments show, animals with biliary fistula can live even for years, the function of this fluid in digestion must, at all events, be a limited, and probably only an indirect one; and this is confirmed by the fact, that the secretion of bile does not attain its maximum till twelve or fifteen hours after food has been taken, when by far the greatest part of the ingesta must have passed beyond the doudenum; hence the greatest part of this fluid enters the small intestine at much too late a period to exert in it any great influence on the metamorphosis of the chyme.

[From trials upon various animals, it has been ascertained that although the *pancreatic juice* exerts no solvent action in albuminous bodies, it yet acts with extreme power in converting starch into dextrine and sugar.]

Bernard, about four years ago, discovered that remarkable fact, that, externally to the organism, the pancreatic juice possesses the property of decomposing the neutral fats into their base and acid.

The only unquestionable action of the paecreatic fluid, is that which it exerts on starch; but since the relative size of the pancreas is greater in carnivorous than in herbivorous animals (the weight of this gland being one-three-hundredth that of the whole body in dogs and cats, and only one-six-hundreth in that of rabbits,) and since, further (as has been shown by Bidder and Schmidt's experiments,) the greater part of the amylaceous food of the sheep is converted into sugar before it enters the duodenum, we may fairly conclude that the principal uses of this secretion are still unknown.

There are far greater difficulties in obtaining pure *intestinal juice*, than in procuring any of the secretions we have previously considered. The most recent investigations on this subject, are those of Frerichs, contained in his article on Digestion, and of Zander, a pupil of Bidder and Schmidt's, who have themselves continued and extended his researches, and their conclusions are, upon the whole, very discordant.

All that we can say with certainty with regard to the physico-chemical properties of the fluid is, that after filtration it is a tenacious, ropy, colorless, and strongly alkaline substance.

According to Frerichs, the intestinal juice exerts no change on any of the ordinary elements of food. Protein bodies and gelatigenous substances remained perfectly unchanged; fat became disintegrated in the same manner as in all other viscid fluids; neither, according to Frerichs, does it exert any special action on starch. Hence he denies to the intestinal fluid any action as a direct digestive agent. Lehmann, on the other hand, found that the intestinal juice possessed, in a high degree, the power of converting starch into sugar; but protein bodies and fats were so little affected by it, that he doubts whether it exerts any digestive action on these substances, and the more so, since cubes of coagulated albumen and pieces of flesh, when introduced into the gut of a hospital patient through a fistulous opening, were expelled from the rectum almost entirely unchanged. The fistula was, however, in the lower part of the ileum, and probably near the cæcum. Finally, the results of experiments on nineteen cats and two dogs have convinced Zander and Bidder and Schmidt, that the intestinal juice exerts a solvent action on albuminous bodies, scarcely inferior to that of the gastric juice, and a sugar-forming power on starch scarcely inferior to that of saliva or of pancreatic juice.—*Brit. and For. Med.-Chir. Review, July.* 1853, *p.* 171.

Art. II.—*Organic Chemistry.*

Crystallization of the Blood.—A new and very interesting subject has lately been more formally brought before the profession, we refer to the fact of the production from the blood in certain circumstances, of red "albuminous crystals," which though formed of animal matter, and sometimes, in all probability, during life, have yet forms as regular as any inorganic crystals. An excellent epitome

of the facts bearing upon this matter has been published in the *Medico-Chirurgical Review* for October, from which we select as much as appears calculated to be interesting to the majority of our readers.

It has been hitherto held to be the rule, that crystallization is peculiarly a property of inorganic chemistry, and that crystaline forms are met with in the animal economy only when the material giving rise to them has lost all claim to be considered as an integral part of the living tissues. The discovery of the crystallizing power of the albuminous elements of red blood (*hæmatoidin* of Virchow,) has, however, made it probable that there are exceptions to this general law.

Various authors, as Sir E. Home, Scherer, etc., had described reddish crystals in blood which had been effused into tissues or organs; but Virchow was the first who paid particular attention to their actual nature, as different from saline or earthy crystals. He considers that the formation of these red crystals is the terminal step of the transformation of the coloring matter of the blood, which, first diffused, passes into the granular, then into the crystalline form; the crystals being yellowish or red, of a rhomboidal form, occasionally as large as the ordinary triple phosphate crystals in the urine, and forming spontaneously in extravasated blood, in the course of from a fortnight to three weeks after its escape from the vessels. Virchow concludes that the crystallizing process may take place—1, in the cells; 2, in amorphous protein substances (in both cases from diffused coloring matter;) or 3, from agglommerated and subsequently metamorphosed blood-corpuscles; that the crystallizing power is inherent in the albuminous matter or protein; and that the coloring matter in the crystals bears no other relation to them than the metallic colors impregnating its crystals do to quartz. Reichert comes to similar conclusions; that the crystals are albuminous, and that the coloring matter is not an essential feature. Kölliker has advanced another step in this investigation, having detected the crystals in *the interior of unbroken blood-corpuscles*. In the blood of a dog's liver, he found a great number of blood-globules, containing from one to five rodlets of a dark yellow color; and, in unchanged corpuscles, from the splenic blood, the gradual formation of one or two of the crystals could be followed. Dr. Funke introduces the matter under yet a new aspect; he has succeeded in forming the crystals from the blood. If we add water to a drop of blood spread out upon the object-glass of the microscope, when the drop has just

began to dry up, the edges of the heaps of blood-corpuscles are seen to undergo a sudden change. A few corpuscles disappear, others receive dark thick edges, become angular and elongated, and are extended into small, well-defined rodlets. In this manner, an enormous quantity of crystal embryos are formed, which are too small to enable us to determine their shape; they rapidly extend more and more lengthways, the entire field of vision being gradually covered with a dense network of acicular crystals crossing one another in every direction, other crystals presenting the form of rhombic plates.

The colored rhomboidal crystals, however, as originally described by Virchow, do not appear to be capable of being artificially formed.

Such, we think, is a correct, though brief statement of the principal facts on this very interesting subject.

On referring to the drawings of some of the principal forms which we have already described, we think no one can fail to be struck with their great resemblance to the various crystalline forms found in the urine, uric acid, oxalate of lime, triple phosphate, nay, even cystine. Had we been shown the drawings without being told of their origin, we should at once have set them down as illustrations of the crystalline urinary deposits.—*Asso. Med. Journal*, Nov. 18, 1853.

Milk, and its Constituents.—This secretion has not met with that amount of attention, lately, that its importance, and the means for investigation now at the command of the physiological chemist, would seem to invite. Before noting, however, what has recently been done in this division of organic chemistry, it may not be amiss to detail a few of the accepted facts respecting this liquid, since these may lead to a readier comprehension of what will follow.

First, then, as to colostrum, and the distinction between it and milk; in colostrum, there is not merely an increase in the amount of the saline constituents to twice or thrice of that existing in healthy milk, nor a general augmentation of the solid constituents only, in the ratio of about 17¼ to 11, and which is, according to Simon, principally due to the increased quantity of sugar, although this point is not absolutely determined; but the difference mainly depends on the presence of granular masses, termed *colostrum corpuscles*, which are invariably present in the colostrum, but disappear, as a rule, in three or four days after delivery, being apt to re-appear on the supervention of any acute disease. These colostrum corpus-

cles are much larger than the true fatty milk globules, and consist of small fat globules imbedded in an albuminous substance; whilst, as they exist in larger quantity in the colostrum, than the milk globules do in healthy milk, the former secretion is really richer, than milk is, in fat.

In healthy milk the amount of butter varies according to circumstances, but appears to average nearly 3½ per cent.; this butter is considered to be richer in olein than the butter from cow's milk. The valuable observation of Simon may be remembered by some, that the butter undergoes no appreciable change in amount during suckling, but that the milk-sugar diminishes with the growth of the child; whilst the proportion both of the casein and the saline constituents are augmented. L'Heritier formerly detailed some singular analyses of the milk from two women, both twenty-two years old, one dark, the other fair, from which it appears that not only the quantity of butter, but of almost all the other constituents also, was nearly doubled in the milk of the *brunette;* this assertion is well worthy of a farther investigation to support or confute so singular a statement. In ordinary cases, milk contains about 3½ per cent. of casein, and 4 to 6 per cent. of sugar; one peculiarity about the casein being, that it is less readily coagulated by acids than that from cows' milk. The above remarks apply especially to human milk.

The existence of albumen, or an analogous form of free casein coagulable by heat, in healthy milk, is disputed by M. Lieberkuhn, who has recently investigated this subject as well as the relations of casein and albumen to potash. He finds that when albuminate of potash is gently evaporated on a water-bath, it gradually decomposes into potash, and albumen in an insoluble state; and that the corresponding potash compound from milk, by a similar treatment, undergoes a similar decomposition, separating into alkali and an insoluble skin of casein. With perfectly fresh milk the results are precisely similar, the coagulum being insoluble in boiling water. The substance noticed by Scherer, existing in milk, susceptible of coagulation, and having an albuminous reaction, is not only obtained by adding excess of lactic or acetic acids to milk, and heating the filtrate, but also from the filtered liquor of boiled milk similarly acidified. Boiled milk, when evaporated, as well as fresh milk, when concentrated at a temperature of 104° F., and the residium treated with cold water in both cases, yields a liquid which coagulates on boiling. From these conditions, M. Lieberkuhn contends that milk

contains neither albumen, nor free casein analogous to it, coaglulable by heat. This chemist farther observes that the nitrogenous snbstance contained in fresh milk exists under three distinct forms; one corresponding to albuminate of potash, another coagulable by heat, and a third separable from the serum by filtration. He also points to the probable identity of albumen and casein, but abstains from the avowal of any decided opinion on this point.

In addition to the chlorides of sodium and potassium, the alkaline and earthy phosphates, (bone earth,) and the alkali combined with the casein in milk, Dr. G. Wilson, pursuing the track of some experiments of Mr. Middleton on the constant presence of fluorine in bones, recognized, some two or three years since, the existence of this element in cow's milk, cheese, and whey, existing probably as fluoride of calcium. We do not *know* that it is present in human milk, nor are we aware that experiment has yet shown that this, although most probable, is an actual and ascertained fact. Fibrin, hæmatin, urea, etc., have been recognized in milk; but these are the products of disease, and cannot be reckoned among its constituents.—*Asso. Med. Journal*, Oct. 28, 1853.

Infusoria in Woman's Milk, by Dr. Vogel.—No general directions can be given as to whether a woman may suckle or not. In every case the question must be determined by an examination of the milk; and here the microscope proves eminently useful. The author found in that milk which produced sickness in the child, and destroyed the health of the mother after prolonged lactation, immediately after its removal from the breast, infusoria similar to those found in the incrustations upon the teeth (*vibrio bacillus.*) Such vibriones are found especially in women who menstruate or suffer from hemorrhages during this period, the good or bad aspect giving no important indication. The milk has often a fine thick white color, or is of paler hue; its consistence may be either thick or watery; its reaction is often alkaline, but generally neutral. Under the microscope it exhibits, according to its richness, sometimes but few, at other times many, milk and cream corpuscles; these differ from the corpuscles of healthy milk by their pale yellow color, their want of metallic lustre, and their speedy decomposition. As regards the infusoria, they are little rod-shaped bodies, dark in the middle, surrounded by a lighter line, but exhibiting neither head nor tail under a magnifying power of 600 diameters; there are, however, feet in

great number and of considerable length. The movement of these animalcules was swimming, and occasionally it was very active. Forward movement was worm-like, and an annular structure of four rings was observed. Mostly they twist, screw-like, upon their axes. When they swim in a circle, they always move from right to left. The length 1–100 mmtr., their breath four times less. They are best seen when the milk is diluted with water. In ammonia, diluted acids (even the lactic,) they die immediately.

Children fed upon milk containing these infusoria, become sooner or later attacked by diarrhœa, and the evacuations are of a green color. This condition disappears as soon as healthy cow's milk is substituted. The author believes that this effect does nor proceed from the infusoria as such, but from the same cause which produces the infusoria, namely, a process of fermentation in the milk itself. The ferment is, according to him, the congested and increased heat in the breasts, connected with the general excitement of the sexual system.

But a fermentation as Jul. Clarus observes, cannot be present, because the author always found the milk alkaline or neutral, and never sour. Were there fermentation, the evolution of lactic acid would, upon the author's own showing, have immediately destroyed the infusoria.—*Med. Times and Gaz.* Nov. 19, 1853, from *Schmidt's Jahrb.* 1853.

Color of the Urine.—"I imagine," says Prof. J. Vogel, of Giessen, "that the coloring matter of the urine is formed by the coloring matter of the blood, and that it consists of a decomposition of blood-disks. If this be the case, and it be premised that the decomposed blood-disks do not leave the body through any other channel than the urinary, it follows that we possess, in the color of the urine, a ready means of ascertaining, in a given time, whether a large or a small quantity of blood-disks are decomposed in the animal economy. In the normal condition, two to six parts of coloring matter are voided daily, and this expresses the normal loss and reproduction of blood-disks. A chlorotic patient, or a convalescent, who voids but little coloring matter in the urine, would have but little change and reproduction of blood-disks. Every one whose urine is high colored would require a constant supply of red blood-corpuscles. The grounds upon which this hypothesis rest are the following:

"I. In all cases of brown or brownish-black urine, where the urine is most colored, the direct source of the coloring matter, namely, from decomposed blood-disks, can be easily traced.

"II. In all cases where there is a constant discharge of red urine with increase of the coloring matter, as in fevers, inflammation, etc., there we see, as a common result, diminution in the quantity of the blood-corpuscles, and a corresponding condition of anæmia.

"III. In cases where there is noticed diminution in the production of red blood-corpuscles, with probability of their very sparing disintegration, as in many (not all) cases of anæmia and chlorosis, there do we find diminution in the excretions of the coloring matter of the urine."—*Med. Times and Gaz.*, Oct. 8, 1853, from *Vogel and Nasse's Archiv.*

ART. III.— *On the Physiology and Disease of the Pancreas*—By DR. BRIGHT and M. BERNARD.

[In 1832, three papers were read successively before the Medico-Chirurgical Society on this obscure but interesting subject; the authors being Dr. Bright, Mr. Lloyd, and Dr. Elliotson. These gentlemen directed attention to a peculiar and rare symptom.]

"The symptoms to which I refer," said Dr. Bright, who was the author of the first communication, "is a peculiar condition of the *alvine evacuation;* a portion more or less considerable assuming the character of an oily substance resembling fat, which either passes separately from the bowels, or soon divides itself from the general mass, and lies upon the surface, sometimes forming a thick crust, particularly about the edges of the vessel, if the fæces are of a semi-fluid consistence; sometimes floating like globules of tallow which have melted and become cold; and sometimes assuming the form of a thin fatty pellicle over the whole, or over the fluid parts in which the more solid figured fæces are deposited. This oily matter has generally a slight yellow tinge, and a most disgustingly fœtid odor."

The importance of Dr. Bright's views, as bearing upon recent investigations, will be seen in the following summary of them. He says:—

"When we draw a comparison between the three foregoing cases, a very close analogy, or even identity, in many circumstances, may be traced. In all of them chronic ailment terminated, sooner or

later, in jaundice; and in all of them a great peculiarity in the character of the dejections existed. In the result of the examination after death, we have likewise some circumstances which coincide in all—*obstructed biliary ducts; the liver gorged with bile; fungoid disease* attacking the *head of the pancreas;* and *malignant ulceration* on the surface of the *duodenum.* The question to be solved is, upon which of the conditions indicated or caused by these morbid changes, if upon either, the peculiarity of the evacuations depended? That the obstruction of the biliary ducts, or even the total absence of all indication of biliary secretion, is not usually attended by the same peculiarity in the evacuations, many cases which have been cautiously detailed by various authors, and many which we have all observed, bear sufficient testimony; and I was therefore induced to ascribe it either to the existence of malignant disease, or to that disease being situated in the pancreas. That the simple fact of malignant disease existing, is not *necessarily* productive of such appearances in the feculent matter, I infer from cases both of that form of disease and of melanosis in the liver to a very great extent being, within the scope of my experience, unaccompanied by any such discharge, though the evacuations were submitted to the most rigid observation. That simple ulceration in the bowels, to any known extent, is not attended by any such symptom, I am led to believe, from knowing that neither in the most extensive ulceration of the large intestines in cases of dysentery, nor in the worst cases of ulceration of the small intestines in fever, in diarrhœa, or in phthisis, does anything of the kind usually occur. Whether, however, malignant ulceration of the mucous membrane is accompanied by this symptom, I cannot assert, though I have often seen most extensive ulcers of the pylorus and of the rectum, where, although the evacuations were attentively observed, such fatty matter was not detected. As, however, a malignant ulceration of the membrane did exist in each of the foregoing cases, it is not impossible that this was the cause of this symptom; but we must bear in mind that such ulcerations are *by no means* uncommon, and that the phenomenon of which I am speaking is uncommon; and that in each of the cases it was accompanied by another morbid appearance, which is not common—namely, the malignant disease of the pancreas. The fact of the intestinal ulceration having, in each case, occupied the duodenum, does, however, somewhat diminish the weight of this observation, for that certainly is not so frequent an occurrence."—*Brit. and For. Medico-Chir. Review, July,* 1853, *p.* 154.

ART. IV.—*On Excision of the Knee-Joint.*—By R. J. MACKENZIE, Esq.

[Not only has an unfavorable impression been produced against this operation, but severe censures have also been passed upon the operators. Mr. Park, of Liverpool, in 1783, first prominently noticed the operation, but it had previously been performed successfully by Mr. Filkin of Northwich. After these, Moreau operated in three cases, two of which were fatal. In 1809, Mülder operated upon a pregnant female—she died some months after from tetanus.]

The next notice of the operation is by Mr. Crampton, in 1823. The operation in Mr. Crampton's first case can scarcely be said to bear on the question of excision of the knee-joint; not only were the patella and part of the tibia removed, but upwards of six inches of the thigh bone were taken away. The patient, however, lived upwards of three years after the operation, and then died from phthisis. Mr. Crampton, in his remarks on the case, says, that it "was one to which the operation of excision was not applicable." The disease had proceeded too far; for, even had it been possible to have removed the whole of the diseased bone, and that union had taken place between the femur and tibia, the limb, from its shortness, would have been useless. The operation was performed in the same year, in a second case by Mr. Crampton. The girl, although a most unmanageable patient, made an excellent recovery. Six months after the operation, the femur and tibia were consolidated by a firm bony union. Eight months later, she is reported as being able to walk a distance of nearly five miles at once; and upwards of three years after the operation, Mr. Crampton says:—"I have examined the limb, and find that the femur and tibia are firmly consolidated; the leg and thigh are not in the slightest degree wasted, but the limb is considerably bowed outwards; she wears a shoe with a cork sole four inches thick; and, to use her own expression, 'is able to stand or walk the length of a day.'"

The operation was next performed by Mr. Syme, who, in 1829 and 1830, excised the knee-joints of two children, of seven and eight years of age. Up to this time the operation had only been performed in adults. The result in the second case was unsuccessful, the child having died about ten days after the performance of the operation. Writing in 1831, Mr. Syme gives the following sat-

isfactory account of his first case :—"In the course of four weeks after the operation, the wound was all but healed, and the limb, before the expiration of three months, had regained so much strength, that the patient could make some use of it in walking. It has been progressively improving since, and is still doing well. I have no doubt that ultimately it will be nearly as useful to him as ever ; but even at present he would be very sorry to exchange it for a wooden one. He can walk and run, though with a halt, without the constrained appearance of a person with an artificial leg, and merely requires the *heel* of the shoe to be two inches higher than the other. The limb is stout and well nourished, and though slightly bowed outwards, does not occasion any disagreeable deformity ; it allows a slight degree of flexion and extension." Seventeen years later, however, a much less satisfactory account of the same patient is given. In speaking of excision of the knee-joint, in 1848, Mr. Syme says :—"I tried the operation nearly twenty years ago on a boy, who recovered perfectly from it, and seemed at first to possess a limb little inferior to its fellow, except in so far as it was stiff at the knee. But in the course of time it was found that the growth of the two limbs was not equal, and that the one which had been the subject of operation gradually diminished in respective length, until it wanted several inches of reaching the ground, when the patient stood erect."

After Mr. Syme's experience in 1830, the operation seems, as far as I can ascertain, to have been abandoned till within the last three years. The results of the operation on the whole had been decidedly unsatisfactory.

In 1850, the operation was revived by Mr. Fergusson ; and within the last three years, the operation has, as far as I can ascertain, been performed thirteen times : in three cases by Mr. Fergusson, in six cases by Mr. Jones of the Island of Jersey, in one case by Mr. Page of Carlisle, in one case by Dr. H. Stewart of Belfast, and in two cases by myself. Of the results of these cases, I shall give some details immediately. Of the thirteen three have died, two directly from the effects of the operation, and one from dysentery. The remaining ten are, as far as I know, at present alive. In five the limb is already used freely in progression ; in one the result, as regards the usefulness of the limb, is yet uncertain ; in one, I am ignorant as to the result ; in the remaining three, sufficient time has not yet elapsed to render the result certain, but of each, the most favorable expectations may be entertained.

Such is a brief outline of the history of all the cases in which, as far as I can ascertain, the operation has hitherto been performed, and I think I am justified in saying, that an impartial review of these cases, especially of the later series, at least proves that the propriety of the operation is yet an open question.

[Dr. Mackenzie, after cursorily examining the cases of this operation occurring within the last three years, proceeds to defend those who have been censured for reviving the operation. He says:]

The propriety of attempting to save a limb by excision of the knee-joint, seems to depend on the three following considerations:—1st. Is the operation of excision attended by greater or less danger to life than amputation of the thigh? 2d. In the event of recovery after excision of the knee-joint, is the limb more or less useful and seemly than a wooden leg? 3d. Does the long confinement to the horizontal posture, which is necessary after excision of the knee-joint, in any measure counterbalance the benefits of the operation (if such benefits are proved) as compared with amputation of the limb?

A careful consideration of these questions, and of the recorded results of the operation, led me several years ago to doubt the propriety of the operation having been rejected from surgical practice. I satisfied myself, by repeated trials on the dead body, that the operation could be performed without difficulty, and without the risk of wounding any important parts; and, as a teacher of surgery, I have been in the habit of mentioning to my pupils, my impression, that the operation had been rejected from practice on insufficient grounds. Knowing, however, the objection of my hospital colleagues to the operation, I never requested their sanction in carrying it into execution, till the late experience of Mr. Fergusson and Mr. Jones appeared to me to warrant my urging the question more closely on their attention, and my performing the operation in the cases which I have related.

With regard to the considerations which I have suggested as principally bearing on the merits of the operation, I would submit,—1st. That the danger of the operation may be reasonably expected to be less than that attending amputation of the thigh. The wound necessary for the removal of the diseased bones is less extensive than the wound of amputation of the thigh, whether performed by the circular or flat operation. The large vessels and nerves are not divided in the operation, the parts involved in the incisions being

principally the integuments and ligamentous apparatus of the joint; the medullary canal of the bone is not laid open, a point which has of late years been suggested as of some consequence in diminishing the dangers of amputation; the shock, which always attends, to a greater or less degree, the sudden removal of a large part of the body, is avoided.

Time and experience, however, can alone determine the comparative danger of the two operations. In the mean time, the results of the limited number of cases in which the operation has been recently practised, prove, as far as they go, that the danger to life is not great; and, unless some unforeseen accident should occur to the cases which are at present in progress of convalescence, that the danger is inferior to that of amputation.

2nd. With regard to the usefulness and the seemliness of the limb, I need only refer to the account I have given of the results of the operation in Mr. Jones's hands, to show that the limb may retain a degree of usefulness which can never be attained by the wooden leg in common use, nor yet by the most expensive and efficient artificial limb which I have ever seen; and if, in the days of Mr. Park, such a result could be obtained as that a man, whose knee-joint had been excised, could run up the rigging and perform all the duties of a seaman, surely, with all the resources of modern surgery, we ought to obtain an equally satisfactory result now.

3rd. The remaining objection to the operation is the tediousness of the convalescence. This, I think, unquestionably detracts, to a certain extent, from the benefits of the operation. I am doubtful, however, after all, whether the recovery can be proved to be on the whole more tedious than the recovery from compound fracture of the leg; in some cases, even the limb seems to have been restored to usefulness, as soon as the patient would have been able to walk on a wooden leg, had the limb been amputated. In Mr. Syme's case, for instance, "in the course of four weeks after the operation, the wound was all but healed, and the limb, before the expiration of three months, had regained so much strength, that the patient could make some use of it in walking."

Mr. Syme, in another place, says, "It ought to be recollected, too, that, though recovery from amputation of the thigh is usually completed in three or four weeks, it is generally *at least as many months* before the patient can rest the weight of his body on the face of the stump, so as to use it in standing or walking."

Another objection has been brought forward against the operation which requires to be noticed, viz: that, when the operation has been performed on children, the growth of the limb has been checked, and that, consequently, from the unequal growth of the two limbs, that on which the operation has been performed, becomes so disproportioned to the other, as to be ultimately useless. The only practical proof of this being so, which I can find, is Mr. Smye's case; and I think it is possible that it might be attributed in this instance to anchylosis not having taken place. Experiments have been performed on animals, to show that bones do not grow when deprived of their heads, but the results do not appear to be conclusive. A few years will be required to confirm this as a valid objection to the operation. Mr. Jones has, in the mean time, made accurate measurements of the limbs of the three boys on whom the operation has been performed, and will have an opportunity of giving further information on the subject hereafter. I have, in an earlier part of this paper, mentioned the fact, that the limb in one of Mr. Jones's cases had already, since the operation, grown considerably, and presented now no appearance of checked growth. As bearing on this question, I would be glad to know if the growth of the upper extremity is checked by excision of the elbow-joint. It is so, as far as I remember to have observed, only in a slight degree. Surely the numerous children who have undergone this operation at the age of six or seven, have not grown to adult age, with the stunted arm and hand of little more than an infant. Supposing, however, that the fact is so—that the limb is so much *respectively* shortened in after years—the objection applies only to the operation being performed on children, and has nothing to do with the question of the propriety of excision in the adult.

I have suggested that an error has, till lately, been committed in the performance of the operation, which has been allowed to interfere most seriously with its result. I refer to *the removal of the patella.* It is said that the patella is generally extensively diseased in affections of the knee-joint, demanding excision or amputation: this, I have no doubt, is an error. The amount of disease to which it is liable, is very limited—in the great majority of cases consisting only in its being more or less deprived of its cartilage, and of a rough, or perhaps carious condition of its articular aspect. The patella is rarely, if ever, the primary seat of the disease in affections of the knee-joint. When the disease commences in the bones, it invaria-

bly does so in the cancellated texture of the head of the tibia, or condyles of the femur. It is extremely rare, that there is such extent of disease in the patella as to prevent its easy and effectual removal by the gouge, the body of the bone being left uninterfered with. The advantage of leaving the patella cannot fail, I think, to be very great. The natural form of the joint is preserved; the attachment of the extensor muscles is left undivided; the wound is less extensive; the annoying tendency to displacement forwards of the end of the femur seems, in a great measure, to be done away with: and there is every reason to believe that the consolidation of the bones will proceed more rapidly, and the limb ultimately be found both more useful, and more seemly than when that bone is taken away. This is a point, however, which remains to be proved. In the two last cases, however, in which the operation has been performed by Mr. Jones and myself, the patella has been left, and the advantages of its having been retained, are already apparent in each case.

I shall not lengthen this paper by describing what appears to me the best mode of performing the operation, but merely mention that the semilunar incision seems to me the most advantageous, as being the smallest by which the joint can be satisfactorily exposed, and as giving rise to less bleeding than the H-shaped incision. There is one point, however, to which I wish to direct attention. It has been recommended that a considerable portion of integuments should be removed by a double lunated incision, to prevent the redundancy of skin which might be expected from the large amount of bone removed in the operation. I believe that this advice, if followed, will lead to great annoyance. I have not removed any portion of integuments in the cases in which I have performed the operation, notwithstanding which, the retraction of the skin was such as to cause, in each case, more or less gaping of part of the wound. Mr. Fergusson mentioned to me, that he had removed a small portion of integuments in the operation, in the case now in King's College Hospital, and that, during the patient's convalescence, he had much reason to regret, having done so, as the retraction of the integuments was such as to leave part of the end of the femur uncovered, and a thin cicatrix only now covers this point of bone.—*Monthly Journal of Med. Science, June*, 1853, *p.* 526.

[At a meeting of the Edinburgh Medico-Chirurgical Society, after a paper had been read on this subject by Dr. Mackenzie.]

Mr. SYME said that nothing could be more praiseworthy than trying to extend the means of affording relief by surgery, but as every step in advance was necessarily experimental, it should be understood that there were two sorts of experiments—the *experientia erudita* and the *experientia vulgaris*. In the former, a process of reasoning led to the expectation that certain causes would be followed by certain effects; but in the latter, there was no such foundation for the procedure, which was executed simply with the view of obtaining results. He thought it needless to detain the society by showing that the only warrantable experiments on the living bodies of human beings were those of the former kind; and would endeavor to explain the considerations which induced him to regard excision of the knee-joint as a dangerous and unprofitable proceeding. The large size of the wound, which was just double that of amputation, and the want of a dependent opening for the matter to escape, must be apt to occasion a profuse and protracted discharge—dangerous directly from its exhausting effects, and indirectly from lighting up disease in the pulmonary or intestinal textures, especially in subjects predisposed to such derangements, as most patients requiring the operation were. The strong and irregular action of the muscles must render it difficult to prevent displacement of the bones, impede their osseous union, and favor anchylosis, when it did occur in some awkward position. The ample experience obtained from excision of the elbow-joint—which he had himself performed little short of a hundred times—showed that many months, or years, or even a whole life-time, might elapse before the wound was so completely consolidated, as not to suffer occasionally from small collections of matter in and about the cicatrix, which interfered little with the patient's comfort, as they did not affect the usefulness of the hand, or the strength of the arm, but would entirely unfit the inferior extremity from being employed as a support for the body. In children, the danger of the operation would be probably less than in adults, and the chance of consolidation greater. But, on the other hand, there was reason to fear that the limb would not grow proportionally, and remain in the shrivelled condition usually consequent upon spontaneous anchylosis. The results already obtained were confirmatory of these theoretical considerations. Although the operation had been limited to cases favorable for recovery, a large portion of the patients, whose fate could be regarded as decided, had perished. In some cases there had been no osseous union;

and in others, anchylosis with miserable deformity. Thus in Sir P. Crampton's only successful case—the famous one of Ann Lynch, who "could walk the length of a day"—it appeared from the bones which were in the Lincoln's-Inn-Fields Museum, that the tibia and os femoris were united at a right angle, so that the progressive motion must have been of a very rare and remarkable kind; while the subject of Mr. Park's never to be too frequently quoted case probably made a better appearance, climbing up the rigging of his ship-like the quadrumanous inhabitant of a tropical forest, than he would have done as a biped on terra firma. He, therefore, did not feel warranted to substitute this operation for amputation, which, when properly performed, relieved the patient with little risk, and enabled him to obtain an adequate support, far more useful to a poor man, and ornamental to a rich one, than could be procured through all the suffering, prolonged treatment, and risk of life attendant upon removal of the articulation. But in saying this, he begged it might be distinctly understood, not as an attack upon any other person, but simply as a defence for himself, for declining an experiment which did not in his mind possess the proper requisites of an *experientia erudita.*—*Monthly Journal of Medical Science.*

Art. V.—*Clinics.*

Statistical Report of the Principal Operations performed in the English Provincial Hospitals during the last Quarter of 1853.—[The subjoined Report includes, we believe, all the principal operations performed during the months of October, November, and December, at the following: The Bristol General, the Cheltenham, the Derbyshire General, the Durham County, the Devon and Exeter, the Hull, the Kent and Canterbury, the Leeds, the Nottingham General, the Sheffield, the South Staffordshire, and the York County Hospitals. We propose to continue the Report quarterly, and shall probably have double the number of hospitals on our next list. With regard to two of the above, the following statements include some operations performed during months a little prior mentioned. This does not, however, in the least detract from the value of the statistics, all done during the periods comprised being in each case included:]

Lithotomy.—Number of cases, 5; all recovered.

Case 1. A boy, aged 5, under the care of Mr. Gregory in the Sheffield Infirmary. He was in good health at the time of the operation, and recovered well afterwards. The calculus was a mulberry one, and weighed 10 grains. *Case* 2. In the Hull Infirmary, under the care of Mr. Craven. The boy was eight years old, and in good health. Mr. Skey's staff (projecting curve, long extremity, and semilateral groove,) was used, and, as Mr. Craven thought, with advantage. Owing to the peculiar dum-bell form of the stone, some difficulty was encountered in its extraction, and the incision of the prostate had to be enlarged. The patient recovered well. *Case* 3. In the Winchester Hospital, under the care of Mr. Mayo, a healthy boy, aged 5, recovered. *Case* 4. In the Winchester Hospital, under the care of Mr. Butler, a healthy boy, aged 7, recovered. *Case* 5. A feeble old man, aged 64, in the Winchester Hospital, under the care of Mr. Wickham. The patient recovered, but the wound was about three months in healing, and some incontinence of urine remained afterwards.

Lithotrity.—One case, after four sittings, has been discharged well. The patient, a man aged 60, was under the care of Mr. Teale, in the Leeds Infirmary.

Herniotomy.—Number of cases, 6; recovered, 1; under treatment, 1; died, 4.

Case 1. A male infant, aged 11 months; hernia congenital, of 10 month's duration, and strangulated 2 days; sac opened. The infant did well for a fortnight, when peritonitis with pleurisy occurred, and death took place on the twentieth day. At *post mortem* the wound was found all but healed; suppuration around the canal of the tunica vaginalis, with acute inflammation of the whole peritoneum, and of both pleuræ. *Case* 2. A boy, aged 6, under the care of Dr. Lunn, in the Hull infirmary, admitted, after five day's strangulation of the bowel, in an almost moribund condition. Sac opened. He slowly rallied from the collapse in which he was, and ultimately recovered. *Case* 3. A man, aged 38, not subject to hernia before; hernia right, inguinal, strangulated 24 hours; sac opened; death 58 hours after the operation; autopsy, acute and universal peritonitis. *Case* 4. Inguinal hernia; sac opened; death (particulars could not be obtained.) *Case* 5. A femoral hernia in a woman in the Kent and Canterbury Hospital, operated on Dec. 30, and remaining under treatment at the time of the report. *Case* 6. A man, aged 55, subject to a right inguinal hernia for 35 years; stran-

gulation 48 hours; sac opened; stricture, a band of omentum; death on third day; autopsy—acute peritonitis, with extensive deposit of lymph, part of the ileum gangrenous.

Amputations.—Number of cases, 30; recovered, 18; under treatment, 8; died, 4.

Thigh, 11: recovered, 5; under treatment, 4; died, 2. Of these, 6 were primary, with 2 deaths, 2 recoveries; 1 in an unfavorable condition, and 1 under treatment doing well; 4 were for old standing disease of the knee-joint, all recovered; and 1 for necrosis of the whole shaft of the tibia, and disease of the ankle-joint, under treatment.

Leg, 12; recovered, 7; died, 2; under treatment, 3. Of these, 2 were primary, with 1 recovery, and 1 still under treatment; 2 secondary, 1 dead, and 1 under treatment; 7 for diseased ankle; 6 recovered, and 1 dead; 1 for conical and painful stump, recovered.

Foot, 1; at the tarso-metatarsal joints, primary, recovered.

Arm, 3, all primary; 2 recovered, 1 still under treatment, but doing well.

Forearm, 3; 2 primary, 1 recovered, 1 under treatment; 1 secondary, under treatment. The last was a case of some interest. The patient, a healthy young man, had received an injury to his hand, in which the palmar arch had been wounded. The surgeon whom he consulted applied pressure and very tight bandages, which induced gangrene. of the whole hand, for which latter he was sent to the hospital, where the amputation was performed.

One of the cases in the above list is a double amputation. A railway guard, aged 25, of robust constitution, was admitted, with both legs smashed, into the Nottingham Hospital, under the care of Mr. Eddison. Immediate amputation was performed; on the right above, and on the left below, the knee. In each stump there was much ecchymosis in the posterior flaps, and parts of that of the left afterwards sloughed. At the time our report was received, (a fortnight after the operation,) the poor fellow, in spite of the very free administration of stimulants, appeared to be sinking.

Deaths.—The following are brief details of the four fatal cases; *Case* 1. A man, aged 53, had his foot contused between the buffers of a railway carriage. There was no fracture or dislocation, but sloughing of the soft parts followed to such an extent that amputation through the leg had to be done on the 14th day. The man sank, apparently from exhaustion, a week afterwards. No lesion of

importance was found at the *post mortem*. *Case* 2. A weakly man, aged 47, met with a severe compound fracture of his leg. Primary amputation was performed, imperfect reaction followed, and death from purulent absorption on the 13th day. *Case* 3. A man aged 63, had his thigh amputated for severe compound fracture of the leg. Extreme collapse was consequent on the operation, and death from exhaustion occurred on the 4th day. *Case* 4. Amputation of the leg for diseased ankle was performed on a feeble and emaciated man, aged 26. Secondary hemorrhage occurred on the 10th day, and death from exhaustion on the 14th.

Ligature, etc. of Arteries.—*Case* 1. In the Leeds Infirmary, in a case of popliteal aneurism in which compression treatment had been tried for a month without success, Mr. Samuel Hay applied a ligature to the femoral trunk. The patient, who was a man, aged 58, recovered. *Case* 2. A man was admitted into the Cheltenham Hospital, in consequence of bleeding from the temporal artery. He had sustained a wound of it ten days previously, which had been treated by compression, and over the seat of which a small aneurismal tumour had formed. During one night, bleeding suddenly occurred, and several pints were lost. Mr. Hartley, the House Surgeon, at once cut down on the vessel, and tied both ends, after which the man did well. *Case* 3. In the South Staffordshire Hospital, ligature of both ends of the ulnar artery, on account of wound; successful.

Excision of Bones, etc.—In the Winchester Hospital, by Mr. Butler, removal of the right half of the lower jaw on account of a malignant growth. Mr. Jardine, the House Surgeon, reports, that the tumor presented the microscopic characters of epithelial cancer, and that it had apparently grown from the periostrum. It had been known to exist for two months. In the course of a month after the operation, and before the wound had healed, it re-appeared, and death resulted three months later. No *post mortem* was obtained. In a case of diseased and dilated antrum, under treatment in the Leeds Infirmary, Mr. Teale removed the front wall of the cavity with bone forceps. The result was favorable.

Tracheotomy.—*Case* 1. A child, aged two and a half, was admitted into the Derby Infirmary, pulseless, with a livid aspect, and seemingly asphyxiated. Examination of the pharynx with the finger detected nothing, and a tube passed down the œsophagus proved

that no source of pressure was lodged there. Mr. Dix, the House Surgeon, accordingly at once opened the trachea. A piece of quill having been passed into the wound, respiration was quickly restored, and the patient rallied. During a slight cough which followed, a wedge-shaped piece of hard boiled beef was brought up. The tube was then withdrawn, and the wound closed with plaster. The child eventually recovered, though at one period in great danger from tracheitis. The wound did not heal for upwards of three weeks. *Case* 2. In the Nottingham General Hospital, Mr. Eddison performed the same operation under somewhat similar circumstances to the above, on account of the impaction of a piece of apple in a child's larynx. The four upper rings of the trachea were opened, and very shortly afterwards the foreign body was coughed up. The patient did remarkably well, and the wound soon healed. It was believed that there was a second portion of apple lodged in the right bronchus, which, as it was never brought up in mass, was probably softened and got mixed with the expectoration. *Case* 3. A man, aged 47, a patient in the Leeds Infirmary, had his respiration much obstructed by the emphysema about the throat, consequent upon a fracture of the thyroid cartillage. Mr. Teale opened the trachea, and a perfect recovery resulted.

Operations for Urethral Stricture.—In a case in the Nottingham Hospital, in which an extensive and cartilaginous stricture existed with several fistulous communications with the urethra, Mr. Eddison performed the operation of the perineal section. The patient has since done well, but is yet under treatment. In a second case, Mr. Eddison opened the posterior part of the spongy portion of the urethra after extravasasion of urine. The penis was sloughing, and the scrotum infiltrated with urine, at the time of the man's admission, and he was extremely depressed. Although free incisions were practiced, and stimulants liberally administered afterwards, yet he sank and died within a few days.

Puncture of the Bladder.—In three cases, all of which proved fatal. In two, it was performed per rectum, on account of impassable stricture. In the third, a man was admitted moribund from long retention of urine; the catheter failing, the bladder was punctured above the pubes, and about a gallon of urine, mixed with pus, removed. The man lived four hours afterwards.

Paracentesis Thoracis.—In one case, for the remains of an empyema, which had been evacuated some months previously. The

first opening had been in front, to the left of the sternum; the second was between the sixth and seventh ribs posteriorly. A fluctuating tumor had presented, and it was simply opened with a lancet. The patient recovered.

Paracentesis Abdominis.—In four cases of ovarian dropsy, and two of ascites. In one of the former, some fluid existed in the peritoneal cavity, as well as in an enormous ovarian cyst; and having been first withdrawn, the walls of the latter were found to be so thick that it was deemed best not to complete the operation. The patient died five days afterwards. In another case, the patient died after the rapid refilling of the cyst; the remaining four have been relieved by the operation.

Excision of Malignant Tumors. Scirrus of the Breast.—Five cases, all of which had recovered. In one, in the Canterbury Hospital, the patient, a married woman, age 42, stated that she had first perceived the tumor while suckling, and within six weeks after delivery. In this instance, a gland in the axilla was also removed. The patient's health much improved after the operation, and the wounds healed well.

Epithelial Cancer of the Lip.—Four cases, all recovered. In one, the entire lip was removed, and a plastic operation for its restoration is contemplated. Another was of interest, from the fact that the same disease had been removed eight years previously. About a year before the disease re-appeared, the man had resumed the habit of smoking.

Chimney-Sweep's Cancer (Epithelial)—Two cases—both recovered. In one, under the care of Mr. Smith, in the Leed's Infirmary, the disease affected the cheek of a chimney-sweep, aged 23, living in a country town. He had the appearance of having been very neglectful as to cleanliness. In the second, the growth affected the scrotum of a sweep resident at Winchester.

Cancer of the Skin.—A case operated on at the Exeter Hospital, Mr. Clapp, the House Surgeon, thus describes: A woman, aged 62, tumor the size of a small orange, somewhat pedunculated, very hard, painful, bleeding, and oozing a thin sanious discharge, of six year's growth; said to have commenced as a wart, having at its base three smaller wart-like ones, attended by cachexia, but with no visceral or glandular disease. The operation proved it to be connected with the skin only; and on microscopic examination, Mr. Clapp

believed it to be an example of epithelial cancer. The wound soon healed, and left a healthy cicatrix. In a case under the care of Mr. Teale in the Leeds Infirmary, an ulcerated epithelial cancer was successfully excised from the scalp of a patient, aged 33.

Melanosis of the Eye.—In the Sheffield Infirmary, Mr. Gregory extirpated the eyeball of a man, aged 43, in whom the disease, which the microscope afterwards proved to be melanotic, had existed for six or seven years. The optic nerve where cut appeared healthy. The man recovered well, and appeared subsequently in excellent health.

Epithelial Cancer of the Labium.—One case, recovered.

Epithelial Cancer of the Penis.—Two cases, recovered.

Excision of Non-malignant Tumors.—*Fatty*, nine cases, all recovered. *Encysted*, five cases, all recovered. *Epulis*, two. *Enlarged Clitoris*, one. *Diseased Bursa*, two. *Parotid Tumor*, one. *Tumor in the Eyelid*, one; all recovered, *Enlarged Testis*, one recovered. This was a singular case. The patient, a young man, of 20, was under the care of Mr. Gisborn in the Derbyshire General Hospital. He stated that his testis had been much enlarged ever since the age of 7; and that some weeks before admission, after an attack of gonorrhœa, it had inflamed and suppurated. It weighed, after removal, a pound and a half; and on making a section of it, there were found in its centre, several distinct masses of osseous deposit inclosed in separate sacs, the largest of which closely resembled a molar tooth, but had the structure of true bone.

Exostosis.—The following occurred in the Kent and Canterbury Hospital; R. L., a robust girl, aged 12, had an exostosis the size of a walnut, growing from almost the whole length of the inner side of the first phalanx of her right middle finger, and another of similar character occupying the whole of the outer side of the second phalanx of the ring finger of the same hand. They had existed as long as she could remember, but had grown more rapidly during the last eighteen months. It was intended to have excised them, but during the operation, the bones from which they grew were found so much involved that amputation had to be performed, in the one case, at the metacarpo-phalangeal joint; in the other, at that of the first phalanx.

Cutaneous Tumors (of a doubtful nature.)—The following occurred, also, in the Canterbury Hospital: A healthy looking girl, aged 20, had on her abdomen, two small, fleshy tumors, very pain-

ful, but not at all tender, one of which, on the left of the umbilicus, resembled a large, rounded nipple, with a dark brown areola; the other, in the left iliac region, was flatter and more irregular in shape; the latter had existed from childhood, the former for about three months. After excision, the nipple like growth had the appearance of firm fat, while the other, which was lobulated, had a bluish color, and was hard and almost cartilaginous in structure. The wound in the iliac region did not heal kindly, but some sprouting granulations of suspicious appearance formed in it, which required to be destroyed by caustics; the other soon cicatrized. *Sero-cystic sarcoma of breast* rom a woman, aged 41, in the Leeds Infirmary, by Mr. Smith. It had been ten months growing, and weighed two pounds and a quarter. Recovered.

Loose Cartilages in Joint.—In the Derby Hospital, Mr. Johnson removed three loose cartilages from the knee of a man aged 56. Two of them about the size of musket balls were excised by a free incision from a sac over the outer side of the joint, and probably not having direct connection with it. The third, in size and shape much resembling a patella, was taken from under the ligamentum patellæ or its inner aspect. The wounds discharged healthy pus for a few days and then closed. The swelling of the knee gradually diminished, and the patient did well.

Necrosis of Bone.—Four cases have been operated on, all of them with great relief.

Radical Cure of Hydrocele.—In five cases, injection with diluted tincture of iodine has been practised. In three, cure ensued, but in the fourth the cure is not yet complete, and in the fifth the patient was lost sight of before the result was certain; he had, however, done well.

Plastic Operations. Harelip.—Four cases; all cured.

Cicatrix of Burn.—Two cases relieved.

Tenotomy of the tendo-Achillis for talipes equinus in five cases, all with success. In a case in the Derby Hospital, under the care of Mr. Gisborne, the tendo-Achillis, the peronei, and the extensors of the toes, were all divided on account of severe equinus. The patient (aged 15) left the hospital two months afterwards, with a much improved foot.

Uterine Polypus.—One case; the tumor, a common fibrous polypus, was removed by ligature, and the patient did well.

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Cutaneous Tumors (of a doubtful nature.)—The following occurred, also, in the Canterbury Hospital: A healthy looking girl, aged 20, had on her abdomen, two small, fleshy tumors, very pain-

ful, but not at all tender, one of which, on the left of the umbilicus, resembled a large, rounded nipple, with a dark brown areola; the other, in the left iliac region, was flatter and more irregular in shape; the latter had existed from childhood, the former for about three months. After excision, the nipple like growth had the appearance of firm fat, while the other, which was lobulated, had a bluish color, and was hard and almost cartilaginous in structure. The wound in the iliac region did not heal kindly, but some sprouting granulations of suspicious appearance formed in it, which required to be destroyed by caustics; the other soon cicatrized. *Sero-cystic sarcoma of breast* rom a woman, aged 41, in the Leeds Infirmary, by Mr. Smith. It had been ten months growing, and weighed two pounds and a quarter. Recovered.

Loose Cartilages in Joint.—In the Derby Hospital, Mr. Johnson removed three loose cartilages from the knee of a man aged 56. Two of them about the size of musket balls were excised by a free incision from a sac over the outer side of the joint, and probably not having direct connection with it. The third, in size and shape much resembling a patella, was taken from under the ligamentum patellæ or its inner aspect. The wounds discharged healthy pus for a few days and then closed. The swelling of the knee gradually diminished, and the patient did well.

Necrosis of Bone.—Four cases have been operated on, all of them with great relief.

Radical Cure of Hydrocele.—In five cases, injection with diluted tincture of iodine has been practised. In three, cure ensued, but in the fourth the cure is not yet complete, and in the fifth the patient was lost sight of before the result was certain; he had, however, done well.

Plastic Operations. Harelip.—Four cases; all cured.

Cicatrix of Burn.—Two cases relieved.

Tenotomy of the tendo-Achillis for talipes equinus in five cases, all with success. In a case in the Derby Hospital, under the care of Mr. Gisborne, the tendo-Achillis, the peronei, and the extensors of the toes, were all divided on account of severe equinus. The patient (aged 15) left the hospital two months afterwards, with a much improved foot.

Uterine Polypus.—One case; the tumor, a common fibrous polypus, was removed by ligature, and the patient did well.

CATARACT.—*By absorption* in four cases, all of them double; doing well, but yet under treatment. *By depression* in six cases, and in five with successful results.

ENTROPIUM.—One case; successful.

STRABISMUS.—Three cases; all successful.—*Med. Times and Gaz.* Feb. 4, 1854.

PART THIRD.

BIBLIOGRAPHICAL NOTICES AND REVIEWS.

Practical Observations on Aural Surgery; and the Nature and Treatment of Diseases of the Ear. With Illustrations. By WM. R. WILDE, Fellow of the Royal College of Surgery in Ireland; Surgeon to St. Mark's Opthalmic Hospital; Honorary Member of the Royal Medical Society of Stockholm, etc., etc. Philadelphia: Blanchard & Lea, 1853. For sale by Riley & Co.

The work before us embraces a little less than five hundred pages, and has been issued from the press in good style.

In offering this work to the profession, the author confidently hopes to occupy a void in our Medical Literature; one, too, that has been long felt, and that no work yet published has seemed adequate to fill.

The principal design of the author seems to be to expose error and establish truth; to lay down just principles for an accurate diagnosis of diseases of the Ear; to rescue their treatment from empiricism, and found it upon the well established laws of modern pathology, practical surgery, and reasonable therapeutics. For doing this, his opportunities seem to have been rare, having been connected for ten years with the management of a large institution in Dublin, for a long time the only one of the kind where clinical and practical instruction in Aural Surgery was delivered in Great Britain.

One of the greatest imaginary difficulties to the student of Aural Surgery, is supposed to be experienced in acquiring a knowledge of the Anatomy and Physiology of the Ear. The minuteness of the organ itself, the difficulty of dissecting it from its depth, the com-

plexity of its structure, and the small hard bone in which it is placed, as well as the number of hard names attached to its different parts, taken all together, seem to constitute an unsurmountable task; and hence, perhaps, the small number of Medical men who feel themselves competent to practice Aural Surgery.

These obstacles, however, are more imaginary than real; a little industry and patience properly directed, will soon unravel its anatomy, and the appearances and movements of its different parts in health, sufficiently to comprehend the diseases to which it is liable. As all know the Ear is early developed, on this account it can be examined very well in fœteses — better, indeed, than in aged subjects. Material of the former kind is not scarce in even country practice, and hence no one can have an excuse for not investigating the subject.

As a specimen of the work, we insert the following:

"Up to a very recent period we possessed no better means of examining the external meatus and the membrana tympani than that afforded by the usual ear speculum, made somewhat in the form of a crane-bill forceps, and derived, with various modifications, from the time of Fabricius Hildanus. Itard, Deleau, Verret, Robbi, Kramer, and others, have improved upon this speculum, which was that in general use in this country until I introduced the tubular form in my Essay on Otorrhœa. Another description, with three arms, and opening by a screw in the form of a vagina speculum, was manufactured by Mr. Weiss, on the supposition that the external auditory passage could be increased in calibre by mechanical means, and Hoffman has published an account of a somewhat similar instrument in "*Casper's Wochenschrift,*" for 1841. In making examinations of the meatus and membrana tympani with any of these instruments, the chief requisite is a *strong direct light,* transmitted without interruption to the tympanul membrane, or that portion of the passage which we wish to examine. This is best effected by means of the sun's rays, but as the ordinary speculum can only dilate or straighten the external cartilaginous portion of the passage, a person accustomed to aural examinations can frequently, especially where the meatus is of a large size, observe the tympanul membrane, or at least a portion of it, without, as well as with, such an instrument, by merely lifting up the auricle with one hand and pressing the tragus forward with the thumb of the other. The light must, however, be strong, and made to fall directly upon the passage. In

organ itself, the difficulty of dissecting it from its depth, the com-

all such examinations, the patient should be seated beneath the examiner, with the head slightly bent, opposite a window *through which the sun is shining* at the moment, and, if possible, between the hours of eleven and three."

This work is a contribution to science, and, inasmuch as the class of diseases of which it treats is likely to fall, for some time at least in our country, into the hands of the general practitioner, it behooves him to avail himself of every source of information at his command.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By JAMES PAGET, F. R. S., Prof. of Anatomy and Surgery to the College; Assistant Surgeon and Lecturer on Physiology at St. Bartholomew's Hospital. Philadelphia: Lindsay & Blakinston. For sale by Randall, Aston and Long.

The Lectures which go to make up this work, were delivered at the Royal College of Surgeons, during the six years from 1847 to 1853, which time the author held the office of Professor of Anatomy and Surgery to the College. Listened to with such favor, the author has, as he says, ventured to give them to the public in a form to be more generally examined.

The design appears to be to give a course of Lectures which will illustrate the *general pathology of Surgcial diseases, in conformity to the larger and more exact doctrines of physiology*. For the purpose of doing this, numerous wood engravings are introduced into the work, that add very much to its value as an elementary treatise.

The question might be asked, what can be said on Surgical Pathology that is different from that contained in the books already before the profession? We answer, *much*. There is scarcely a work, however voluminous, that discusses surgical pathology in such a way as to contribute to correct views on the propriety of Operations. Physiology lies at the foundation of Pathology, and without a knowledge of the former, all the views of the latter are imperfect. Natural processes, together with all the circumstances that affect them in health, must be studied before diseased ones can be appreciated. How many of our surgeons are good physiologists? How many, instead of regarding their vocation as the one set apart for the exercise of the *ultima ratio medendi*, go to work patiently for

however, be strong, and made to fall directly upon the passage. In

the purpose of ferreting out the secret processes of nature? and yet the latter is the only course that gives character to a surgeon. A butcher can cut off a man's leg, adjust the bones of a fractured cranium, or remove a tumor; but it requires a *surgeon* to know when it is proper to do such things, whether or not the patient would be thereby benefitted. Lately attention among surgeons has been awakened to the results following the removal of what are usually termed *malignant growths*, and as a consequence, we have some very respectable monographs on the subject, the tendency of which must be to limit the scalpel to a nanauer the sphere of action. In our opinion it requires more sense to know when an operation should *not*, than it does to know when it should be performed.

The plan pursued in the work before us, is, in the first place, to present and discuss such subjects as *Nutrition*—its nature, purpose, and conditions, in order to prepare the reader to appreciate what is said on departures from the healthy standard. The following may be regarded as a specimen of the style of the author, and of his doctrines:

"In the natural course of life, the formative process manifests itself in three modes, which, though they bear different names, and are sometimes described as if they were wholly different things, yet probably are only three expressions of the same law, three effects of the same force, operating in different conditions. The three, enumerating them in the order of their time, are *development*, *growth* and *assimilation*, or maintainance.

"By development we mean generally the process by which a tissue or organ is first formed, or by which one as yet imperfectly formed is so changed in shape or composition as to be fitted for a higher function, or finally is advanced to the state in which it exists in the most perfect condition of the species.

"We must carefully distinguish development from mere increase; it is the acquiring not of greater bulk, but of new forms and structures, which are adapted to higher constitutions of existence. For example, when in the embryo groups of nucleated cells are changed into muscular fibres, there is not necessarily an increase in size; or if there be, there is something more; there is a change of texture and an acquirement of power adapted to a higher state of existence; these constitute development, so when, from the simple cavity and walls of the embryonic digestive system, the stomach, intestines, liver pancreas, and other organs are produced, these are developed,

there is increase; but at the same time something more than mere increase."

In the elucidation of his subject, the author has brought to his aid the use of the *microscope*; the work, as a consequence, abounds in nnmerous microscopical diagrams, illustrating almost every variety of morbid growth which usually falls within the range of surgery. On the subject of *Cancer*, the microscopic illustrations are very copious, and well calculated to facilitate diagnosis. In describing the microscopic appearances of *hard* Cancer, the author remarks:

"In very thin sections, it is not very difficult to see the infiltrations, or insertion of the cancer substance in the interstices of the affected tissues. It may be most clearly seen in affections of any part of the skin, recently invaded by the cancer, for here, in the meshes of the reticulated fibro-cellular and elastic tissues, the cancer particles are quite distinct, filling every interval, and not obscured by the debris of the gland-ducts and their contents. I am not aware of any more orderly plan of arrangement of the materials of the cancer, than that which may be expressed by saying that they fill the interstices when they accumulate quickly and abundantly; or, when they shrivel and degenerate they may allow the tissues to collapse or contract.

The elementary structures of the cancer substance thus infiltrated, in the breast for example, are chiefly two: namely, 1. Certain cells and corpuscles, and 2. a fluid or solid blastema or merely homogeneous substances in which these lie imbedded."

Here, for want of space, we must close our notice of this valuable work. Any medical library is incomplete without it, while to the surgeon it is indispensable.

Pneumonia. Its supposed Connection, Pathological and Etiological, with Autumnal Fevers; iucluding an Inquiry into the existence and Morbid Agency of Malaria. By R. La Roche, M. D., Member of the American Philosophical Society of the American Medical Association; Fellow of the College of Physicians of Philadelphia, etc., etc. Philadelphia: Blanchard and Lea, 1854. For sale by Riley & Co.

This work, the author intimates, has not been got up for the purpose of adv..ncing and supporting any new theory, with refer-

ence to the principal subject discussed, but to collate within a small compass all the facts known to the profession with regard to one of the most frequent diseases, and its supposed connection with Autumnal Fevers.

The author combats what we suppose nobody with sense believes, viz: that there is a necessary connection between our Autumnal fevers, intermittent remittent, and Pneumonia. "The idea," he says, "of a close connection, as regards both causation and nature, between thoracic inflammations and malarial fevers of various grades and types, has long been entertained, and continues even now to be advocated by writers of respectable standing."

To our mind, the above statement contains a slight, or rather a gross misapprehension. No educated man with whom we have ever talked, either North or South, doubts that intermittents and remittents are the offspring of a distinct specific cause, exising in districts of peculiar characters; nor that Pneumonia is connected, intimately in its origin with climate, vicissitudes of temperature, etc. That the two classes of disease often prevail at the same season, and that the same individual is frequently the subject of both at the same time, are facts that have not escaped the attention of any one, however stupid. But the frequent connection of the two diseases in the same person, has not been referred to as evidence of identity of cause by any one whose observations are entitled to respect; and hence, we think the principal effort of the book before us has been directed against a phantom.

In malarious regions, Pneumonia, as all know, is of frequent occurrence, particularly during certain seasons of the year. Not unfrequently the pathological elements of this complaint are very much modified by malarious influences. Most, if not all the symptoms, assume a periodical character, and the case, which was at first amenable to evacuants and antimony, after a while requires anti-periodics, such as quinine, arsenic, etc.

A reason for the frequency of pneumonia in malarious districts, may be sought for in the fact that such districts lessen in a very sensible manner every vital function of the organism, and as a consequence predispose the inhabitants not only to pneumonia, when the season for its prevalence arrives, but also to epidemics of every kind. These epidemics are frequently modified in a manner similar to what has been found to be true of pneumonia; but nobody has thought it necessary to caution us about regarding them as being identical in cause with intermittents and remittents.

What we have said is not designed to disparage the work. It contains five hundred pages of matter relating to the two classes of diseases, and so much has been brought forward with reference to the character of each, that no one who has not had his attention specially turned in the same direction, can rise from its perusal without feeling benefitted.

To Charles D. Meigs, M. D., the author has thought proper, in a very modest and well written letter, to dedicate the work—a compliment to Dr. M. well merited, if industry and zeal have anything to do with such things.

Clinical Report on Chronic Pleurisy, based on an Analysis of forty-seven cases. By Austin Flint, M. D. Professor of Practical Medicine in the University of Buffalo, New York, and in the University of Louisville. Buffalo: Printed by Jewett Thomas & Co., 1853.

The Report covers some fifty-eight pages, and like most of what comes from the author, exhibits industry applied in the right direction. An analysis of forty-seven cases of chronic pleurisy, it is true, may not lead to the establishment of a single fact, yet it is nevertheless the only plan by which *true* facts can be established. The natural history of the disease under consideration is a fruitful theme, and one which has previously been too much neglected. Although not of very frequent occurrence, it nevertheless is not so rare, but what it falls occasionally under the observation of most practitioners. In most instances, the author thinks it is not recognized. Indeed, Hope also thinks that there is no class of affections more frequently overlooked than this. The essay before us, will improve the diagnosis.

In the *treatment*, the author among other things, makes some remarks on *paracentesis thoracis*, as devised by Dr. Wyman of Massachusetts, when effusions are troublesome. This method consists in using an exploring canula and trochar, which is attached by a flexible tube to a suction pump, so constructed, that the fluid may be removed from the chest, through the canula, and discharged from the pump by another aperture. This method is considered an important improvement in the management of chronic pleurisy. (For sale by H. C. Morton, Louisville, Ky.)

PART FOURTH.

EDITORIAL AND MISCELLANY.

The circulation of the blood—New motive powers.

In our exchanges from the North, and also from the South, we notice that considerable inquiry has been excited in regard to the circulation of the blood. The subject also lately came up in the Drake Society of this City, and occupied some three or four evenings in its discussion.

A knowledge of Harvey's discovery seems, at this day of progress in physiological matters, to be nothing more than the mere alpha of the process. The normal amount of the blood, its chemical composition, the capacity of the central organ, and the vessels connected with it, to contain and propel the fluid to the various parts of the organism; and the probability that other forces, more potent and better adapted to the purpose, are concerned in the process, are questions that are now exciting attention as being of especial interest to the physiologist, pathologist, and practicing physician.

The principal effort of those who have brought the matter forward for consideration at the present time, seems to be, to show that there are other forces, besides that of the heart, to which the movements of the blood are due. The sum of these, Dr. Cartwright proposes to express by the term, Hœmatokinety. To the introduction of this term into physiological science, a formal protest has been entered in certain quarters. Among others who reject it, and mostly for whose benefit it has been supposed it was coined, is Mrs. Willard of Troy, New York, who has made herself quite notorious as the alleged discoverer of what she regards a "*New Motive Power.*"

She insists on the truth of the following doctrines, which we have thrown into form as a kind of substratum for her theory.

1st. That in the process of respiration, combustion takes place from the presence of the oxygen of inhaled air, and the carbon of venous blood, and caloric, as a consequence, is set free; 2d. About a fifth part of all the blood in the system is in the lungs, and about seven-eighths of this is water. 3d. The temperature of the blood in the lungs is about thirty degrees higher than is necessary to convert

water *in vacuo* into vapor, and the blood in the lungs is mostly *in vacuo;* 4th. Such being the case, a portion of water in the blood is there changed to vaper, and the volume of the blood becomes so expanded, that it must move. From the influence of the valvular system, the course is directed to the left side of the heart. Possessed of great irritability, this organ as soon as touched by a warm fluid beats, and by its valvular arrangement, the current is propelled forwards.

Dr. Cartwright, who is supposed to have exhibited some sympathy for this theory, has been held up, by several writers of some ability, and formally ridiculed. This has awakened in the Dr. a disposition to defend himself, and as a consequence, he reviews the whole subject connected with the motive powers of the circulation, and makes some experiments, an account of which is published in the New Orleans Medical and Surgical Journal, that are among the most striking on record.

It is perhaps generally known that Prof. Draper, in a *treatise on the forces, which produce the organization of plants,* puts forth a physical principle, which is appealed to, by those who are incredulous in regard to the agency of the heart, in capillary circulation. It seems it is susceptible of demonstration, that "if two fluids communicate with one another in a capillary tube, or in a porous or parenchymatous structure, and have for that tube different chemical affinities, movement will ensue; that liquid which has the most energetic affinity will move with the greatest velocity, and may even drive the other liquid before it." Now then, that arterial blood, charged with oxygen with which it is ready to part, and being prepared to receive in exchange carbonic acid, which the tissues set free—must obviously have a greater affinity for those tissues than venous blood, in which both these changes have already been affected. (Carpenter.) The blood, upon mere physical principles, which enters the systemic capillaries on one side of the net work, must as a consequence drive before it, and expel on the other side the blood which has become venous, while traversing it. Of course, in the pulmonary capillaries opposite affinities would prevail.

Such is the principle, and such the explanation of the circulation by capillary *attraction* and chemical *affinity*.

In support of this theory, much has been brought forward in regard to the circulation of *sap* in vegetables, and the circulation of the blood in inferior animals, in neither of which, is there any cen-

tral propelling organ. Take as a sample the experiments of Hales: "In experiment XXI, I exposed one of the chief roots of a pear tree in full growth, at a depth of two and a half feet, cut off the point of it, and connected the part of the root left in connection with the stem with a tube, which was filled with water, and closed it with mercury. In consequence of the evaporation from the surface of the tree, the root absorbed the water in the tube with such force, that in six minutes the mercury rose eight inches in the tube. This is equal to a column of water nine feet high."

In some of the lower classes of animals, there appears to be an absence of any central propelling organ. This is true of all of two divisions of the animal kingdom—the *Animalia Radiata*, and Animalia Articulata. Their organs of respiration are almost universally seated on the surface of the body, and they possess no organ similar in function to that of a heart. The same thing, is *in part* true, of several classes of Moluscous and vertebrated animals. In these, although, there is a central organ, it seems not to be constructed for propelling purposes.

With reference to all these facts and principles, it may be stated, that they go to make it very plausible, that there are other forces besides those of the heart, to which we might look for an explanation of the chief motive power of the circulation, provided we had no heart. But inasmuch as nearly all vertebrated animals possess this organ, and inasmuch as it is highly endowed with contractile powers, which, according to the experiments of Hale's, is adequate to sustain a column of water 7½ feet high, the weight of which would be 4 lbs. 6 oz.; and inasmuch as it appears to be beating away all the time in the chest, in a series of well defined rythmical contractions, that with many, are the *sine qua non* of vitality, it might not be a very unwarrantable physiological conclusion to assign it some little place in the list of motive powers.

That however, the heart with all its power, its *vis a tergo* and its *vis effronte* is adequate to explain every thing connected with capillary circulation; the circulation in *erectile* tissues, the *portal* circulation, the circulation which presents itself within the cranium, &c. &c., is a position to which no one can yield his assent, when all the facts are carefully examined.

Medical Heresies, now as in times past, continue to take up considerable space in our Medical Journals. But this is not all, books are every now and then written with great care and accuracy, for their exposure and refutation, still the vice continues as rife now as formerly, and in some forms has a deeper hold upon community, than at any former period.

Perhaps it would be well enough now to pause and consider what has been the effect of these attempts to put down quackery. *Can it be that they have coincided with the vice?*—that they have been the principal means by which it has been kept alive? Simpson publishes a work in Edinburgh, Scotland, in refutation of the tenets of Homœopathy. It is reviewed elaborately in the leading journals of Europe; then republished in this country, and again reviewed by most of our journals. The effect of all this is to *advertise* Homœopathy—to make hundreds and thousands know that such a thing exists, who otherwise would never have heard of it. Besides the very fact of noticing it, gives it consequence, and excites the passion for new things in the multitude. They do not stop to enquire into its claims upon reason or common sense, but decide to give the system a trial. Once in for humbuggery of any kind, a taste for things of that character is developed, and those who find out at last, that they have been deceived by any one of them, instead of returning to science, seem the more eager to embrace another.

Again, are these heresies originated, and submitted to the people, as an experiment upon their disposition to patronize them, or are they the result of a demand for such things, proceeding from defective medical knowledge, and deficiency of the reasoning powers, when applied to things medical? If the latter, the remedy will not be found to consist in making a fuss with those who are ministering to the cravings of the depraved appetite, but in the administration of such things as will cure the appetite itself.

We need no more books, such as Simpson's and Hooker's, written to prove the inefficiency and imposition of the various empyrcisms of the day, for if this were done, with all the certainty of demonstration, the results would be the same. What we want is something to dissipate the ignorance, the almost universal ignorance which obtains in regard to the different branches of science, which make up medical science. Any one who believes that this can be brought about, many look forward to the time when quackery will disappear from the land. It will require nothing short of a levelling

up of the masses, including clergymen, lawyers and politicians to the standard of common sense and science. As things are at present, "the credulity of dupes is as inexhaustable as the invention of knaves" (Burk.)

Female Physicians. — The *Boston Female Medical College* has graduated the following young ladies: Miss S. Fletcher, Lowell, Mass; Miss L. A. Harris, Waterville, Maine; Miss Mary R. Jenks, Springfield, Mass.; Miss M. N. Thurston, Mass.

In these days of progress, and the rule of Young America, we are almost prepared for anything, however much opposed to the prejudices of education. Not that we believe all that is going on is right, or will be ultimately sanctioned by the common sense of society. The present, indeed, is an age of *toleration*. It permits not only innovations upon usages and institutions that have been long established, but provides also an open theatre for the introduction and rivalry of every species of charlatanry. Whether this is right or not, we have no disposition to argue. In regard to one thing, however, we have great faith; it is that the chaff will, in time, be separated from the wheat, and that reason and propriety will ultimately prevail.

In nothing, however, that we have said, do we design to be understood as taking grounds against the introduction of females into the medical profession. We only intend that our remarks all be interpreted to suggest that the attempted innovation has happened at a time when humbugs are tolerably rife.

The avocation to which the softer sex aspires, of course has been thoroughly surveyed, in all its departments; and, if any one would become so far lost to prudence as to betray any misgivings that the cost had not been fully counted, and should in his verdancy suggest that the duties of the medical profession are arduous, mentally and physically, more so than can be endured for any length of time by a well constituted male, of course he would be set down as a simpleton, a *ninny*. As a consequence, I suppose, some of the editors of our Journals have either remained silent spectators of what was going on, or taken grounds in favor of the enterprize, and expressed a conversion, to the fullest extent, in the doctrine of "Woman's Rights," as has been done lately by the editor of the Nashville, Tenn.,

Medical Journal. We have glanced slightly at the syllogisms by which the editor to which we have alluded has been convinced of the errors of his former life; and by way of bolstering him up, we can assure him that they are very much like those of Miss Lucy Stone, in a lecture delivered a few evenings since in this City on 'Woman's Rights.' As far, however, as we have been able to comprehend, these syllogisms did not agree in all respects exactly with the *dictum de omni et nullo* of Aristotle and Whately. Still these men are *old Fogies*, perhaps not always to be trusted, particularly when they differ in logic with other personages of distinction!

From the representation of those who are engaged in advocating the doctrine of Woman's Rights, it might be presumed that the *males* have been treating the females about in the same way that Russia has been treating Poland, and, as a consequence, the peace of society is about to be disturbed, and the whole country plunged into a "sexual" war. Should such unfortunately prove to be the case, of course a field of usefulness will be opened to our friend of the *Nashville Journal*, and in the event of an engagement between the belliggerents, he may be looked out for in the costume of a Captain.

Books Received.—We have received a copy of the "*Types of Mankind*," illustrated by selections from the inedited papers of Samuel Geo. Morton, M. D., and by additional contributions from Prof. L. Agassiz. L. L. D., W. Fisher, M. D., Prof. H. S. Patterson, M. D. By J. C. Nott, M. D., and Geo. R. Gliddon: Philadelphia, Lippricut, Grambo & Co. For sale by Riley & Co.

Also, Richardson's Human Anatomy: Philadelphia, Lippricut, Grambo & Co. For sale by Riley & Co.

The above works were received too late for review in our present number. They will be attended to in our next.

Fever a cure for Insanity.—In the annual report of the Superintendent of the Ohio Lunatic Asylum, we see it stated that 31 cases of *typhoid* fever occurred in the Institution during the past summer. Of this number, one terminated fatally; and *thirteen* were restored to reason concurrently with their convalescence from the fever.

TABLE showing the weight of a fluid oz. of urine in grains for specific gravities between 1000 and 1041; also the amount of solids and water in each fg. ʒ; als othe solids in a pint, in apothecary's measure, similar to the tables of G. Bird, as given in Imperial measure.

By THEO. G. WORMLEY, M. D., of Columbus, O.

Sp. gr.	weight of fl oz. in grs	Solids in fl oz. in grains.	Water in grains.	Soilds in fl oz. in grs.	Sp. gr.	weight of fl oz. in grs	Solids in oz. in grs.	Water in fl oz. in grains.	Solids in fl pt. in grs.
1000	455,6944	.....	455,6944	.....	1021	465,2641	22,7654	442,4987	364,2464
1001	456,1501	1,0628	455,0873	17,0048	1022	465,7198	23,8798	441,8470	381,9648
1002	456,6058	2,1278	454,4780	34,0488	1023	466,1755	24,9823	441,1932	399,7168
1003	457,0615	3,1948	453,8667	51,1168	1024	466,6312	26,0940	440,5372	417,5040
1004	457,5172	4,2640	453,2532	68,2240	1025	467,0869	27,2078	439,8791	435,3248
1005	457,9729	5,3354	452,6375	85,3664	1026	467,5426	28,3287	439,2189	453,1792
1006	458,4286	6,4088	452,0198	102,5408	1027	467,9983	29,4418	438,5565	471,0688
1007	458,8843	7,4844	451,3999	119,7504	1028	468,4549	30,5619	437,8921	488,9904
1008	459,3400	8,5621	450,7779	136,9936	1029	468,9097	31,6842	437,2255	506,9472
1009	459,7957	9,6419	450,1538	154,2704	1030	469,3654	32,8086	436,5568	524,9376
1010	460,2514	10,7238	449,5276	171,6808	1031	469,8211	33,9352	435,8859	542,9632
1011	460,7071	11,8079	448,8992	188,9264	1032	470,2768	35,0638	435,2130	561,0208
1012	461,1628	12,8941	448,2687	206,3046	1033	470,7325	36,1946	434,5379	579,1136
1013	461,6185	13,9824	447,6361	223,7184	1034	471,1882	37,3275	433,8607	597,2400
1014	462,0742	15,0726	447,0014	241,1648	1035	471,6439	38,4625	433,1814	615,4000
1015	462,5299	16,1654	446,3645	258,6464	1036	472,0996	39,5997	432,4999	633,5952
1016	462,9856	17,2601	445,7255	276,1616	1037	472,5553	40,7390	431,8163	651,8240
1017	463,4413	18,3569	445,0844	293,7104	1038	473,0110	41,8804	431,1306	670,0864
1018	463,8970	19,4558	444,4412	311,2928	1039	473,4667	43,0239	430,4428	688,3824
1019	464,3527	20,5569	443,7958	328,9104	1040	473,9224	44,1696	429,7528	706,7136
1020	464,8084	21,6601	443,1483	346,5616	1041	474,3837	45,3178	429,0659	725,0848

Method of making a quantitative analysis of the amount of oxalate of lime ($CaO, CO \times 2Aq$) *found in urine.* *By* Theo. G. Wormley., *M. D., Columbus, Ohio.*

We first change the oxalate of lime into the cloride of calcium, and then pursue a process similar to that of "alkalimetry," that is, find the amount of oxalic acid that the chloride thus obtained will saturate.

Before we can apply this test, we must know the exact value of our oxalic acid, and this we may do by exposing pure crystalized oxalic acid ($HO, CO \times 2Aq?$)$_2^{23}$ in a water bath, and carefully raising it to a temperature of about 150° until it crumble to a dry powder;

we thus drive off the water of crystalization, and it will then have the formula $HO, \underset{23}{CO}$, the atomic weight of which is 45. If we dissolve one grain of this acid in a cubic inch, of water contained in a tube graduated to hundreths, each measure will indicate 1–100 of a grain of oxalic acid.

We next proceed to change the oxalate of lime found in the urine, into the chloride of calcium, (CaCl) which may be done by the following process:

Filter the urine through the best filtering paper, taking care to first moisten the filter with pure water. After the urine has passed through, wash the filter; there will then remain upon it all of the *oxalate of lime*, together with crystals of *ammonia phosphate of magnesia*, $(2MgO; \underset{4}{NH}, O, PO \bowtie 12\,Aq)$ *vesicle mucous, epithelial scales*, and perhaps crystals of *uric* $\underset{5}{acid}$ and *cloride of sodium*. Dry the filter and digest it in dilute hydrocloric acid, which will dissolve the oxalate of lime, $(\underset{23}{CaO, CO} \bowtie HCl = CaCl \bowtie HO, CO)$ together with the triple phosphate, and perhaps part of the vesicle $\underset{23}{mucous}$, the epithelial scales and uric acid will remain undissolved; by passing through a filter we will get rid of the insoluble portion, and have an acid solution of the salts of lime and magnesia, together with the free oxalic acid liberated from the oxalate of lime; add excess of ammonia, which will neutralize the free hydrocloric acid, and at the same time decompose the chloride of calcium, forming chloride of ammonium, and oxalate of lime; $(CaCl \bowtie HO\underset{23}{CO} \bowtie \underset{3}{NH} = CaO, \underset{23}{CO} \bowtie \underset{4}{NH}, Cl)$ we also cause the precipitation of the ammonia phosphate of magnesia now add excess of acetic acid, $(HO, \underset{4\,3\,3}{CHO})$ which will dissolve the precipitated salt of magnesia, but leave the oxalate of lime undissolved; by filtration we separate the magnesian salt and retain the lime upon the filter; wash the filter, dry and maintain it at a red heat for several minntes in a platinum crucible. The oxalate will become changed into the carbonate of lime; $(\underset{23}{CaO, CO} = CaO, \underset{2}{CO} \bowtie CO)$ act upon this with dilute hydrocloric acid which will form chloride of calcium, again filter, neutralize with ammonia, which will perhaps cause a slight precipitate; if so, filter. We will now have an alkaline solution of chloride of calcium, the atomic weight of which is 55. We now add to this our solution of oxalic acid drop by drop, until it ceases to give a precipitate, which may easily be ascertained by placing a drop of the filtered fluid under the microscope and adding a

drop of the oxalic acid solution; if it yields a precipitate, we still add more of the acid to the original solution.

From the amount of acid required to neutralize, we ascertain the amount of chloride of calcium present by the following equation:

45: 55:: a (the amount added) : x = (the amount of chloride present.) Then by taking the atomic weight of chloride of calcium (55) for the first term, and the atomic weight of oxalate of lime ($CaO, CO_3 + 2Aq = 82$) for the second, and substituting the value of x we will have

$$55 : 82 :: x : z.$$

The value of z will be the amount of oxalate of lime that was present in the specimen examined.

To illustrate by a case of actual analysis which required 13 measures to neutralize, we had the equation

$$45 : 55 ::, 13 :, 158,$$

which represented the chloride of calcium present. Then

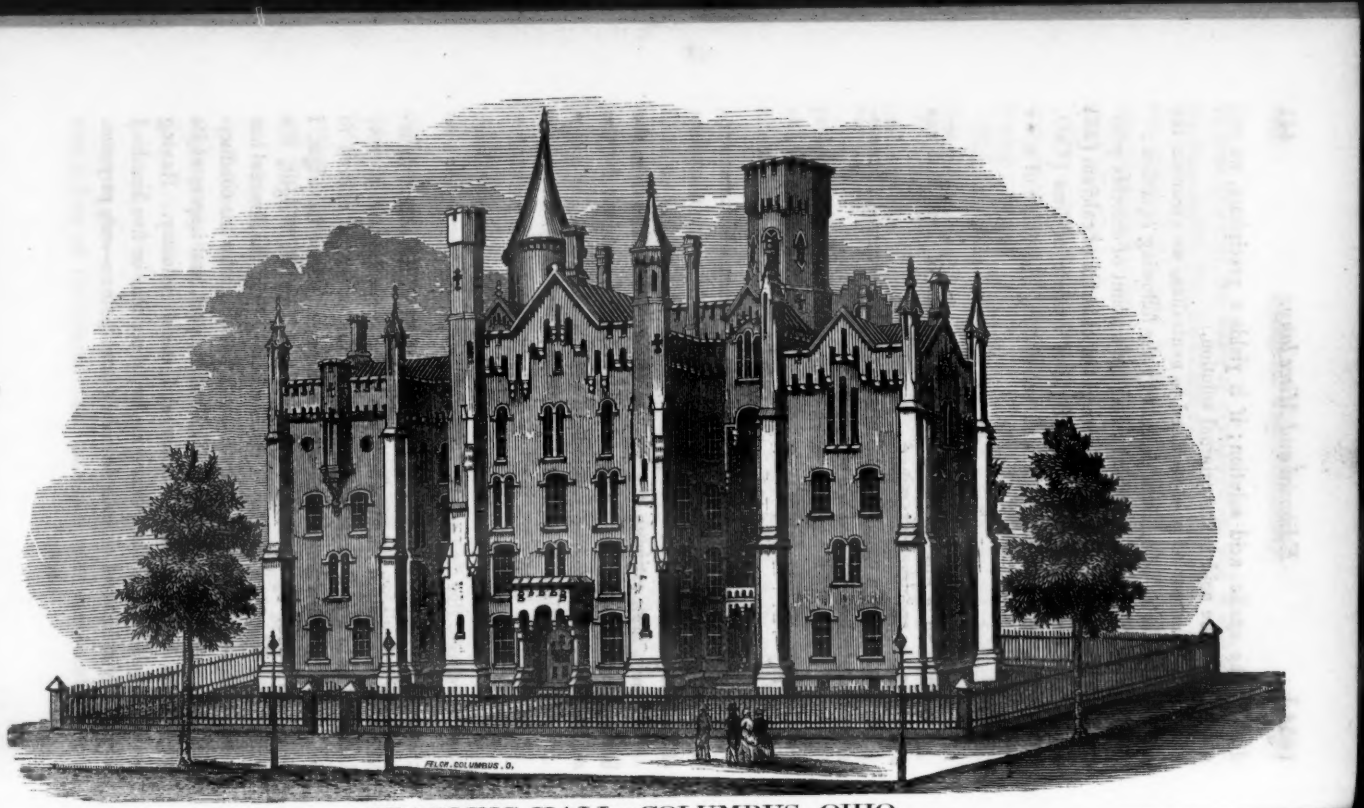
$$55 : 82 ::, 158 :, 235,$$

which was the amount found in six ounces of urine, which was one-fourth of the amount voided in one day, making, 94 grains of oxalate of lime passed during that time.

We may, also, estimate the amount of oxalate of lime from the first equation, by substituting the atomic weight of that salt for the second term, as

$$45 : 82 ::, 13 :, 235.$$

Western Journal of Med. and Surg.—This, the oldest Journal of the Mississippi valley which has had a permanent existence, comes to us in a new dress, and we see by the cover, is offered to subscribers at $3 a year, *invariably in advance.* Since its origin, I believe in 1828, by the late Dr. Drake, and Guy W. Wright, the former of whom for a number of years was its editor, the Journal has been conducted with great ability. Prof. Yandell, who has conducted it for a number of years back, is still at his post, and impresses its pages with his learning, accuracy and usual industry. Having written for this Journal in years gone by, some four or five hundred pages, we can but feel an interest in its future fortune—*esto perpetua.*



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